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ACQUISITION OF AVOIDANCE WITH A VARIABLE CS-US INTERVAL[1]

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THE EFFECTS OF VARYING the time interval between CS and US on the acquisition of avoidance responding have received considerable experimental attention (Warner, 1932; Kamin, 1954; Brush, Brush, & Solomon, 1955; Church, Brush, & Solomon, 1956). These various studies, however, each employed only a single CS-US interval with a given subject: that is, although experimental groups differed with respect to CS-US interval, an individual subject was exposed invariably to the same CS-US interval. The role in avoidance learning of the temporal relation between CS and US can be explored in a very different way. With each subject, varying CS-US intervals can be scheduled in a predetermined random sequence. The performance of subjects treated in this way can be compared with that of those trained with fixed CS-US intervals. The comparison bears on the question of the degree to which acquisition of avoidance depends upon a precise temporal discrimination by the subject, as opposed to the establishment of more general aversive properties of the CS. Traditional studies of conditioning and of avoidance learning employ a fixed CS-US interval. The present study employs the variable CS-US interval procedure.

METHOD

Subjects and Apparatus

The *Ss* were 30 experimentally naïve hooded rats, about 100 days old. The rats, maintained on an *ad lib.* feeding schedule, were randomly assigned to experimental groups. The apparatus was a modified Mowrer-Miller shuttle-box, more fully described in an earlier report (Kamin, 1957a). The US was 1.1 ma. electric shock. The CS was a 74 db. Edwards Lungen buzzer.

Design

There were two basic experimental groups of 15 *Ss*. The variable CS-US group, during the course of training, was exposed to CS-US intervals of 3, 6, 9, 12, and 15 sec., randomized with the restriction that each CS-US interval occur four times within each block of 20 trials. The fixed CS-US group consisted of five squads of three *Ss*. Within a given squad, the CS-US interval was *either* 3, 6, 9, 12, or 15 sec. Thus, in comparing the variable and fixed CS-US groups, the total number of

[1]This research was supported by a grant from the Associate Committee on Applied Psychology of the National Research Council, and facilitated by a University College grant-in-aid from McMaster University.

scheduled exposures to each CS-US interval is the same for each group. Within the variable group, each *S* was scheduled to experience equally often each CS-US interval; but within the fixed group each *S* experienced only one CS-US interval.

Procedure

The basic procedure was the standard shuttle-box avoidance training described in detail elsewhere (Kamin, 1957a). There were 100 trials in a single experimental session, with an inter-trial interval of 1 min. The procedure was delayed conditioning, with response termination of both CS and US on escape trials and response termination of the CS on avoidance trials. With the variable CS-US group, the CS-US interval in effect for a given trial depended upon a schedule determined before the experiment. Thus a response with a given latency might be an avoidance or an escape, depending upon the CS-US interval predetermined for that trial.

Measures

The frequency of escapes and avoidances was tabulated, and response latencies were recorded by stop watch to the nearest .1 sec. The frequency of "spontaneous" inter-trial responses was also recorded. With the variable CS-US group, avoidance data were separately tabulated for each of the five values of scheduled CS-US interval. The full CS-US interval was not experienced, of course, unless *S* failed to avoid. Thus, from *S*'s point of view, classification of a trial within a particular CS-US interval is often arbitrary. To illustrate, *S* might have avoided on each of two trials with a latency of 2.5 sec. The events impinging on *S* were the same on each trial, but *E*'s schedule might class one as a 3 sec. and the other as a 15 sec. CS-US interval.

Results

The data for various indices of acquisition, for the two main experimental groups, are presented in Table I. (The between-rat variability, and small *N* within each squad of the fixed CS-US group, make a precise analysis by squad unfeasible. There were, as might be expected (Brush, Brush, & Solomon, 1955), no easily detectable trends associated with CS-US interval.) The differences between the variable and fixed CS-US procedures are small, and, submitted to *U* and *t* tests, in no case approach significance. This is true with regard both to appearance of the first avoidance, and to the maintenance of avoidance.

Within the variable CS-US group, we can examine acquisition as a function of scheduled CS-US interval. The number of presentations of each class of CS-US interval is no greater for the variable than for the fixed CS-US group, but between-subject variability does not enter into a test of the effect of scheduled CS-US interval under the variable procedure. The mean percentage of avoidances, as a function of blocks of 20 training trials, is presented for each CS-US interval of the variable procedure in Figure 1. The curve for the 3 sec. interval seems to fall below all others. This apparent tendency was tested by an analysis of variance of total number of avoidances, with subjects and scheduled

TABLE I

VARIOUS INDICES OF ACQUISITION, BY EXPERIMENTAL GROUP

Measure	Experimental group	
	Variable CS-US	Fixed CS-US
Total number of avoidances		
Median	68.0	62.0
Mean	62.1	50.1
Range	7–86	6–88
Median latency of avoidance		
Median	2.60″	2.80″
Mean	2.67″	3.17″
Range	1.20–5.00″	1.20–4.90″
Trial of first avoidance		
Median	10.0	14.0
Mean	13.1	22.3
Range	6–31	3–79
Latency of first avoidance		
Median	4.20″	3.40″
Mean	4.98″	3.56″
Range	0.90–10.80″	0.80–7.50″
Spontaneous responses		
Median	8.0	6.0
Mean	11.7	12.9
Range	1–38	0–41

CS-US interval as effects. The subjects × CS-US interval residual was employed as an error term, resulting in an F for CS-US interval of 28.57, 4 and 56 df, $p < .001$. Tukey's gap test differentiates ($p < .001$) the 3 sec. CS-US interval from all others, which form a single cluster. The mean percentages of avoidances for the whole of training were, as a function of increasing CS-US interval, 42, 63, 67, 69, and 69. Thus, within the variable procedure, there was a very clear tendency for the subject to make fewer avoidances when the shortest CS-US interval was scheduled. There was no such tendency evident within the fixed CS-US procedure. The mean percentages of avoidances within the fixed procedure, again presented in order of increasing CS-US interval, were 51, 45, 67, 56, and 31.[2]

DISCUSSION

The data indicate, first, that avoidance responding can be efficiently acquired and maintained when the temporal interval between CS and US varies unpredictably over a quite broad band of values. The variable CS-US procedure produces at least as much avoidance as does a fixed

[2]The small number of avoidances made by the 15-sec. Ss prompted the running, after the main experiment, of two additional Ss trained with a fixed 15 sec. CS-US interval. The mean percentage of avoidances for the five 15 sec. Ss was 47, quite in line with the above figures.

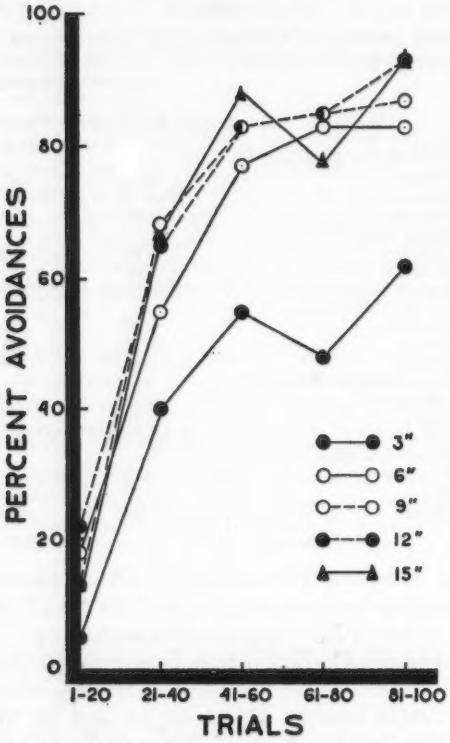

FIGURE 1. Acquisition of avoidance as a function of scheduled CS-US interval.

CS-US interval. There is thus little ground for supposing that a temporal interval *per se* is utilized by the rat as a cue in the acquisition of avoidance.[3] There exist only fragmentary data on the effects of a variable CS-US interval in classical conditioning (Bersh, Schoenfeld, and Notterman, 1953), but the present results imply that the Pavlovian "inhibition of delay" hypothesized by Brush, Brush, and Solomon (1955) is not a necessary pre-condition for acquisition of avoidance with a delayed conditioning procedure.

[3]The fixed inter-trial interval does not, at least within a bank of 100 trials, allow rats to form a temporal discrimination. With the same apparatus, 100 trials of avoidance training under a trace-conditioning procedure—employing a fixed inter-trial interval—yield virtually no avoidance responses.

Previous studies have demonstrated that response terminations on escape trials of CS and of US are each factors in producing the appearance of anticipatory avoidance (Kamin, Campbell, Judd, Ryan, & Walker, 1959); and that, once avoidance occurs, response termination of the CS independently reinforces avoidance (Kamin, 1957b). The present data indicate that these effects are not dependent on any regular temporal interval between CS and US. That is, mere pairing (in a forward direction) of CS and US seems a sufficient condition to endow the CS with aversive properties, and to make CS termination a reinforcing event.

When avoidance has once begun under the variable procedure, the subject's behaviour tends to convert the predetermined schedule to one involving fairly regular CS-US intervals. The frequency and the median latency of avoidance are such that, with training well under way, the longer CS-US intervals are actually experienced only rarely (cf. Figure 1, Table I). The most frequently experienced CS-US interval later in training is 3 sec. Perhaps the median avoidance latency and the proportion of avoidances made at each CS-US interval are functions of the particular set of CS-US intervals employed in the schedule, but there is no evidence on this point.

The fact that temporal cues seem unimportant for the establishment and maintenance of avoidance for a bank of 100 trials does not, of course, mean that they would remain so if training were continued indefinitely. With a very different experimental technique, Sidman (1955) has already demonstrated the importance, after prolonged training, of a time discrimination based upon a fixed CS-US interval.

Summary

The performance of 15 rats given shuttle-box avoidance training with a variable CS-US interval was contrasted to that of 15 rats trained with comparable fixed CS-US intervals. There were no significant differences between the two training procedures, indicating that a time discrimination based on a regular CS-US interval is not involved in the development of shuttle-box avoidance responding. Within the variable CS-US interval procedure, there was a pronounced tendency for fewer avoidances to occur on trials scheduled at the shortest CS-US interval.

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PACING RATE AND WARNING SIGNAL IN SERIAL SIMPLE REACTION TIME[1]

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A PREVIOUS PAPER presented the results of an investigation into the relation between the duration of the foreperiod and the duration of the warning signal in serial simple reaction time (Foley, 1959).

The study revealed that the effective foreperiod started with the onset of the warning signal and that a 2 sec. effective foreperiod resulted in significantly faster reaction times than a 4 or 8 sec. one. A suggestion was also made that a situation, such as that used by Telford (1931), in which the preceding stimulus serves as the warning signal for the next stimulus may be different from a situation in which a separate warning signal is given. In common with other studies (Teichner, 1954), no attempt was made to control the pacing rate. The afterperiod, that is, the interval between the response and the onset of the next warning signal, was held constant at 4 sec. Therefore, each different foreperiod duration resulted in a different pacing rate.

The present investigation was undertaken to determine whether the foreperiod effect previously reported is a true foreperiod effect or a product of the different pacing rates used. Further, if pacing is demonstrated to have no effect, then the relation between warning signal and no warning signal situations can be explored, because only then can a foreperiod of a given duration be compared with a period of the same duration, the period being defined as the interval from termination of a stimulus to onset of the next stimulus.

METHOD

Apparatus

The stimulus and warning signal lights were mounted, one above the other, on a black board which was placed normal to *S*'s line of sight. The top light, red in colour, was the warning signal and the bottom light, green, was the stimulus. The brightness of these lights was approximately 100 ft.-lamberts. Mounted directly beneath the board was a microswitch, the response key.

The control apparatus consisted of three Hunter timers, a stepping switch and series of relays arranged so that the duration of the warning signal, the duration of the foreperiod, and the duration of the afterperiod were independently variable.

[1]Defence Research Medical Laboratories Project no. 107, DRML Report no. 107–9, P.C.C. no. D77–94–20–23, H.R. no. 181.

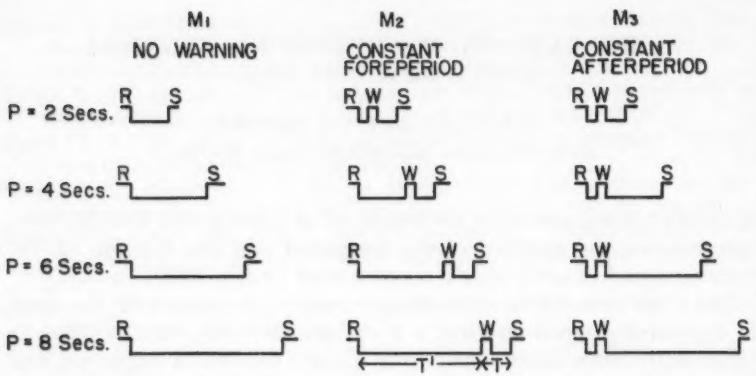

FIGURE 1. Schematic outline of the time relationships involved in the three methods of presentation, where *R* is the response, *S* the stimulus, and *W* the warning light.

Reaction times were measured using a Hunter "Klockounter" calibrated in milliseconds.

Procedure

If we consider the interval from the termination of a stimulus to the onset of the next stimulus in a serial reaction time task to be one cycle, then the period (P) defines the rate at which stimuli are presented. Let T denote the interval between the onset of the warning signal and the onset of the stimulus. Let RT denote the reaction time. Let T^1 denote the interval between the response and the onset of the warning signal for the next response. Then $P = T + RT + T^1$. The value of RT can be ignored in the measurement of P, as being relatively constant and relatively small. These relations are made clear in Figure 1.

In the experiment, P was assigned the values 2, 4, 6, and 8 sec.

For each value of P three methods of presentation (M) were studied, defined as follows:

M_1 where no warning signal was given

M_2 where T was held constant at 1.5 sec., and T^1 assumed the values 0.5, 2.5, 4.5, and 6.5 sec.

M_3 where T^1 was held constant at 0.5 sec., and T assumed the values 1.5, 3.5, 5.5, and 7.5 sec.

The duration of the warning signal in M_2 and M_3 was held constant at 0.5 sec.

Thirty consecutive presentations were made to each S under each condition, the first ten being considered practice trials. The analysis is based on the last twenty.

The design was 4×3 factorial. The 12 possible combinations of P and M were presented to each S in random order during the one session.

Subjects

Nine female Ss, who were paid for their services, were used. Their reported ages ranged from 25 to 35 years. They were instructed to respond to the onset of the

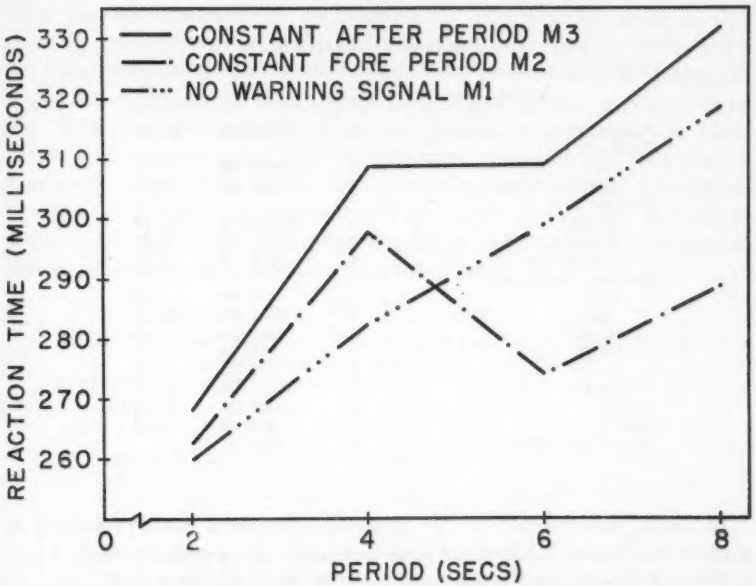

FIGURE 2. Mean reaction times for three methods of serial simple reaction time presentation, as a function of period, where period denotes the inter-stimulus interval.

stimulus light by pressing the response key as quickly as possible. Each used her preferred hand, the responding finger, the index finger, resting on the key.

RESULTS

Figure 2 shows the mean reaction time for each method and for each period. An analysis of variance was carried out, the results of which are shown in Table I. Subject differences are significant, but subject interactions are not, demonstrating that all subjects behaved in the same way during the experiment.

The interaction between methods and periods approaches significance. To examine these effects more precisely an orthogonal breakdown was carried out. The specific comparisons made between methods were 1 *versus* 3 which was not significant, and 1 and 3 *versus* 2 which was significant ($p < .001$), so that the condition of a constant foreperiod was different from the conditions of both no foreperiod and variable foreperiod, and these latter conditions were not different from each other.

The standard components of the period effect were taken out, and the linear component was found to be significant ($p < .001$).

TABLE I

ANALYSIS OF VARIANCE

Source	df	MS	F
Subjects (S)	8	15910.83	29.46
Methods (M)			
1 *vs.* 3 (m_1)	1	3656.55	6.77
1+3 *vs.* 2 (m_2)	1	6585.80	12.19
Periods (P)			
Linear (P_1)	1	28710.94	53.16
Quadratic (P_2)	1	1190.33	2.22
Remainder (P_3)	1	4275.70	7.92
Periods × methods			
m_1P_1	1	8.49	.02
m_2P_1	1	5555.60	10.29
m_1P_2	1	241.63	.45
m_2P_2	1	144.22	.27
m_1P_3	1	613.35	1.14
m_2P_3	1	1113.25	2.06
$S \times M$	16	1046.02	1.94
$S \times P$	24	824.42	1.53
$S \times P \times M$ (Error)	48	540.07	

In testing components of the methods interaction with periods it is evident that Method 2 does not have a common slope with Methods 1 and 3, the m_2P_1 component being significant at the 1 per cent level.

Method 2 was then examined separately with regard to period differences. Essentially it does not differ from a straight line parallel with the x axis, although there is a cubic component which approaches significance. This is no doubt due to the result for the 4 sec. period for this condition.

CONCLUSIONS AND DISCUSSION

When a constant afterperiod is maintained so that differences in foreperiod duration give rise to differences in pacing rate, serial reaction time is affected in that it is a linear function of foreperiod duration or pacing rate. However, when the foreperiod is held constant and pacing rate varied by varying the duration of the afterperiod, no changes in reaction time are apparent, within the range studied. Therefore, it may be concluded that the foreperiod duration is the relevant factor, and not the pacing rate.

The condition where the preceding stimulus is made to serve as a warning signal does not differ from the situation where a separate warning signal is given 0.5 sec. after the previous response. The value of 0.5 sec. for the afterperiod in M_3 was chosen to facilitate comparisons between M_3 and M_2. It may seem that the warning signal comes too soon

after the stimulus-response complex to be differentiated from it. But since a 1.5 sec. foreperiod with a 0.5 sec. afterperiod does not differ from the same foreperiod with 2.5, 4.5, and 6.5 sec. afterperiods, it appears reasonable to generalize from these results. However, taking into account the value for the 4 sec. period, it was decided to investigate in more detail. Five of the original subjects who were still available were run in a separate experiment, under exactly the same procedural conditions, except that T was held constant at 2 sec., and T^1 assigned the values 0, 0.5, 1, 1.5, and 2 sec. The results are shown in Table II. It can be seen that no differences exist between these conditions.

TABLE II

MEAN REACTION TIMES FOR 5 PERIODS WITH DIFFERENT DURATIONS OF AFTERPERIOD* FOR EACH OF 5 SUBJECTS

	Period				
Subjects	2	2.5	3 (msec.)	3.5	4
1	188.2	200.1	193.9	200.5	204.1
2	274.5	287.6	274.3	283.7	279.3
3	233.6	244.3	232.4	239.1	211.9
4	203.9	209.4	209.3	206.5	208.4
5	246.8	283.8	261.9	293.1	298.4
Grand means	229.4	245.0	234.4	244.6	240.4

*Foreperiod was held constant at 2 sec.

A recent experiment by Klemmer (1956) to determine the effects of time uncertainty in simple reaction time has a condition similar to M^1. P had values of 1, 2, 4, 8, and 12 sec. His results for this condition have essentially a common slope with the M^1 results although the points on his curve are approximately 30–40 msec. lower. The common slope shows close agreement between the two conditions. The difference in height can be attributed to the differences between the two groups of subjects, Klemmer using young college males, the present experiment using older females. It should perhaps be pointed out that this is the only similarity between the two experiments. Klemmer's was carried out to explain why an interval should be optimum in terms of time uncertainty. The present experiment was carried out to determine what the appropriate interval is.

The general conclusion can, therefore, be drawn that the effective foreperiod to a stimulus must be defined as the interval from the onset of a preceding stimulus, whether or not this takes the form of a separate warning signal. The rate at which responses are made is not a factor in determining the shortest reaction time.

Summary

A serial reaction time experiment was undertaken to answer the following questions: (1) Is the foreperiod effect a true foreperiod effect, or is it a product of the different pacing rates used? (2) Is a serial reaction time task where the preceding stimulus serves as the warning signal for the next response different from one where a separate warning signal is given?

It was concluded that: (1) the foreperiod effect is a true foreperiod effect; (2) the task with no warning signal does not differ from the task with a warning signal.

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IMPRINTING WITH VISUAL FLICKER: EVIDENCE FOR A CRITICAL PERIOD[1]

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IN A PREVIOUS PAPER (James, 1959), an experiment was described in which domestic chicks were trained to approach a stationary object by placing the object close to a flickering light at one end of a runway. When five days of training had been given in this manner, the object was moved up and down the runway, and it was found that the chicks followed it closely. This behaviour seemed analogous to that observed in conventional imprinting experiments, in which the following response is elicited, without prior training, by exposing the chick to a moving object. It is well known, however, that imprinting can usually be obtained with the conventional method only during a sensitive period of the chick's life. Jaynes (1957), for instance, has found that communally raised New Hampshire chicks will not imprint if they are more than 54 hours old when first exposed to the test object, and Hess (1958) that White Rock chicks raised in isolation fail to do so if they are more than 32 hours old at first exposure. Since the chicks used in the experiment with visual flicker were at least 7 days old when they saw the object move for the first time, it is necessary, if the analogy referred to above is to be entertained further, to show that the attractiveness of visual flicker, like the attractiveness of the moving object in conventional imprinting experiments, decreases with age at first exposure. Such was the main purpose of the experiment described in the present paper.

METHOD

Apparatus

The runway, a 10 ft. long hardboard box, was similar to that described in the earlier paper. A single hole, ¾ in. in diameter, was drilled in the centre of each end wall, 6 in. from the floor. A 100-watt light was placed behind each of these holes, a piece of clear polythene being interposed between the hole and the light to prevent the chicks from pecking the latter. Two silent Hunter timers were used to make one or the other of these lights flicker at the desired on/off rate. A blue polythene beaker, 2 in. in diameter at the top, 1¾ in. in diameter at the base and 2 in. high, was suspended upside down by a nylon thread so that its lower edge coincided with the top of the hole drilled in the end wall of the runway. The beaker could be moved along the length of the runway by pulling on another nylon thread.

[1]This research was supported by grants from the National Research Council and the Committee on Scientific Research, Queen's University.

Subjects

The results reported here are for the 32 survivors of a batch of 38 New Hampshire × Barred Rock chicks obtained from a commercial hatchery. They were isolated from one another immediately on arrival and kept in individual compartments with hardboard walls for the duration of the experiment. Food and water were continuously available. The chicks were approximately 24 hr. old when first introduced into the runway.

Procedure

The chicks were divided at random into four groups. For two of these groups, the light which could be seen through the hole at that end of the runway where the stationary beaker was hanging, was made to flash on and off from the first day of the experiment. In the case of one of them (Group FE ($N = 8$)), the light flashed at an on/off rate of 0.2/0.2 sec.; for the other (Group SE ($N = 8$)), the light was off for 0.2 sec. every 5 sec. The light at the other end of the runway was lit continuously. The chicks in the other two groups were also placed individually in the runway on the first and subsequent days of the experiment, but with the difference that the lights at *both* ends of the runway were lit continuously for the first five days.[2] On the sixth and following days of the experiment, these two groups were exposed to a flickering light at that end of the runway where the beaker was hanging. For one of them (Group FL ($N = 7$)) the light flashed at an on/off rate of 0.2/0.2 sec.; for the other (Group SL ($N = 9$)) it was turned off for 0.2 sec. once every 5 sec. All chicks were run individually for one 5 min. trial a day, and all were given ten training trials in which the stationary beaker was paired with a flashing light. The beaker was present in the runway throughout the experiment, and during the training trials was fixed to the wall immediately above the light-hole in one or the other of the end walls. One of Gellerman's trial orders (Hilgard, 1951) was used to determine at which end of the runway the beaker should hang (and therefore, when appropriate, where the light should flicker) on any given trial.

All chicks were given a test trial after the first five training trials, this trial taking the place of the day's training trial. After all ten training trials they were given a further five test trials. On these test trials the lights at both ends of the runway were lit continuously. The beaker was placed at one end of the runway and then, 30 sec. after the chick had been placed in the centre of the runway, it was pulled 2 ft. down the runway at the rate of approximately 1 ft. every 2 sec. Thirty seconds later, the beaker was moved another 2 ft., and so on until it had been to the other end of the runway and back again to a point 2 ft. from the end at which it had started.

Scoring

At the beginning of each trial the chick was placed in the middle of the runway, and its position was then noted every 30 sec. to the nearest 3 in. It was then scored according to whether the position it occupied at the end of a given 30 sec. period was closer to ("approach") or further away from ("avoid") the beaker than the

[2]In a pilot experiment, the difference between a group of Barred Rocks ($N = 6$) which was adapted to the runway during the delay period, in the manner described here, and a group ($N = 5$) which was kept in the brooder throughout the delay period was found to be insignificant ($U = 14$; $p = 0.5$). Both groups were significantly less responsive than another ($N = 7$) which was first exposed to the flickering light within a maximum of 24 hours of hatching ($U = 8$, $p = 0.04$ and $U = 4$, $p = 0.02$ respectively).

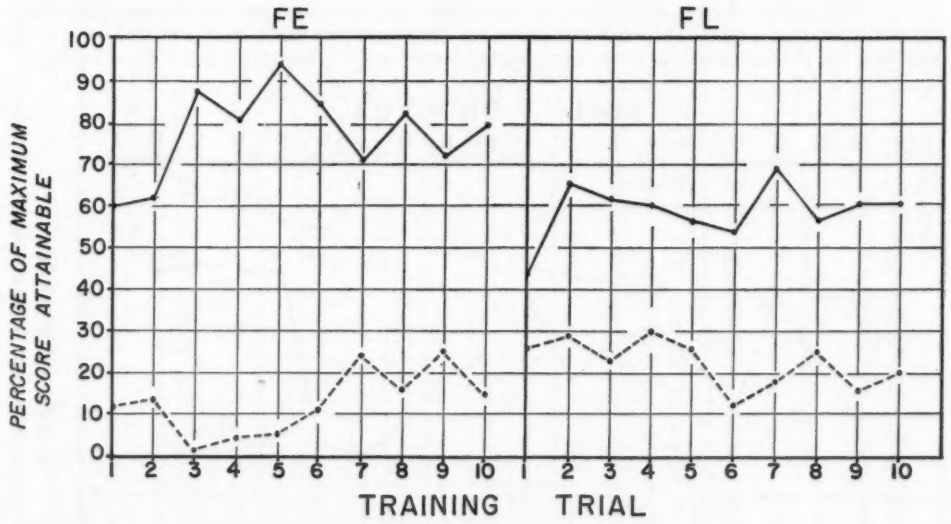

FIGURE 1. Approach (solid line) and avoidance (dotted line) scores of Groups FE and FL, expressed as a percentage of the maximum attainable (10), for each training trial. On Trial 1 Groups FE and FL were approximately one and seven days old respectively. On/off rate of flicker = 0.2/0.2 sec.

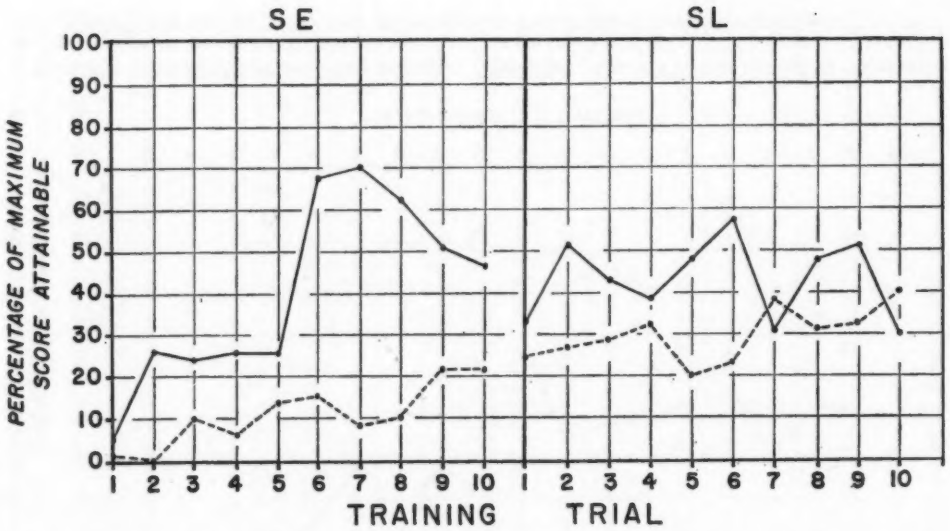

FIGURE 2. Approach (solid line) and avoidance (dotted line) scores of Groups SE and SL, expressed as a percentage of the maximum attainable (10), for each training trial. On Trial 1 Groups SE and SL were approximately one and seven days old respectively. On/off rate of flicker = 5.0/0.2 sec.

position it occupied at the beginning of that period. If it was in the same position at the end of a 30 sec. period as it had been at the beginning of that period, it was scored as "stationary."[3]

Results

Effects of Age at First Exposure

Figures 1 and 2 give the approach and avoidance scores of the four groups during training as percentages of the maximum score attainable (10). If the ratio

$$\frac{\text{Approach score} - \text{Avoidance score}}{\text{Approach score} + \text{Avoidance score}}$$

is taken as a measure of the effectiveness of flicker, the differences between the groups are found to be significant (Mann-Whitney test; Group FE *versus* Group FL: $U = 7$, $p = 0.007$; Group SE *versus* Group SL: $U = 2$, $p = 0.001$). The results of the five terminal test trials[4] are given in Figures 3 and 4, from which it will be seen that, with a fast rate of flicker,

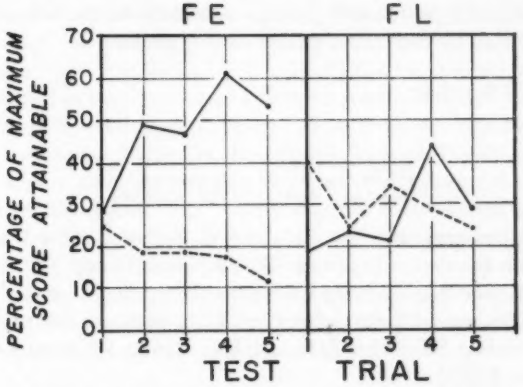

Figure 3. Percentage approach (solid line) and avoidance (dotted line) scores for Groups FE and FL for the five terminal test trials.

the difference is in the expected direction (Mann-Whitney test; Group FE *versus* Group FL: $U = 11$, $p = 0.027$). The difference with age when

[3]If a chick stayed within 3 in. of that end of the runway at which the light was flickering for any 30 sec. period, it was scored as "approach" for that period. Similarly, if it stayed at the end opposite to that at which the light was flickering, it was scored as "avoid."

[4]None of the differences for the single test trial which was given after five training trials was significant.

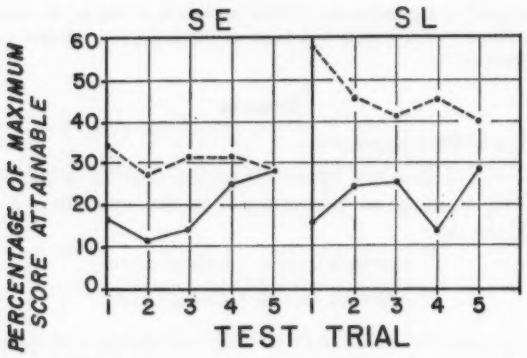

FIGURE 4. Percentage approach (solid line) and avoidance (dotted line) scores for Groups SE and SL for the five terminal test trials.

a slow rate of flicker is used is not significant (Group SE *versus* Group SL: $U = 23$), this presumably being a reflection on the low level of performance shown by both these groups during training.

Effects of Flicker Rate

If the ratio

$$\frac{\text{Approach score} - \text{Avoidance score}}{\text{Approach score} + \text{Avoidance score}}$$

is taken as the measure, the fast rate of flicker is found to be more effective than the slow rate (Mann-Whitney test; Group FE *versus* Group SE: $U = 13$, $p = 0.025$; Group FL *versus* Group SL: $U = 15$, $p = 0.05$). Similar results were obtained during the five terminal test trials (Group FE *versus* Group SE: $U = 7$, $p = 0.003$; Group FL *versus* Group SL: $U = 12$, $p = 0.025$).

Relation between Training and Test Performance

The over-all correlation between the performance of the chicks on the ten training trials and their performance on the five terminal test trials was significant, on a one-tailed test, at the 5 per cent level (Kendall's $\tau = 0.20$, $z = 1.67$).

DISCUSSION

The decreased effectiveness of visual flicker for chicks whose first exposure to this stimulus was delayed until seven days after hatching (Groups FL and SL) is consistent with the hypothesis that there is a

critical period for the acquisition of this response, and to this extent supports the analogy which has been drawn between response to flicker and imprinting. The difference in performance between the two delayed groups suggests that the length of this critical period is, to some extent, under environmental control. This, in turn, suggests that one reason for the shorter critical period which has been found in conventional imprinting experiments (Hess, 1959) may be the relative inefficiency of the stimulation provided by a moving object. A direct test of this hypothesis, however, must await the development of a more reasonable theory of the optical transformations involved than the one which was previously offered (James, 1959).

The low over-all correlation between responsiveness to flicker during training and to the beaker alone during the test trials will not support any speculation on the reasons for the decline in the following response with age at first exposure to flicker. At least some of the variability is due to the inadequacies of the present method of running the chicks. The procedure, for instance, is such that the chicks are "conditioned" not only to the beaker but also to the hole through which the flickering light can be seen during the training trials. Hence on the test trials, when the beaker parts company with the light hole, there is a tendency for some of the chicks either to stay at that end of the runway at which the beaker is hanging when the trial begins, or to follow it to the other end of the runway and then stay there for the rest of the trial. Observation of the chicks' behaviour also suggests that the intervals between the successive movements of the beaker may have been too long in this experiment. Some of the chicks followed it while it was moving, but became disinterested during the interval when it was stationary, and wandered off down the runway. It is, of course, possible that the optimum rate of movement varies with the rate of flicker used during training, but this is a question upon which no evidence is available at present.

Summary

An experiment is described in which differences in the response of domestic chicks to an intermittent light source, and to an object which had previously been paired with this stimulus, were studied as a function of (*a*) their age when first exposed to the intermittent light and (*b*) the rate of intermittency. The main findings were:

(1) The tendency to approach an intermittent light source is weaker in chicks which are exposed to such a stimulus for the first time when they are seven days old than in those whose first exposure occurs within approximately 24 hours after hatching.

(2) Chicks are more readily attracted by a high (on/off rate = 0.2/0.2 sec.) than by a low (on/off rate = 5.0/0.2 sec.) rate of flicker, and are more likely to follow an object which has previously been associated with the higher rate of flicker.

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TRAUMATIC AVOIDANCE LEARNING: THE EFFECT OF PREVENTING ESCAPE RESPONSES[1]

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IN THEORETICAL EXPLANATIONS of avoidance learning with a warning stimulus, attention has been focused on the termination of the warning stimulus as a reinforcer for avoidance (Mowrer, 1947; Schoenfeld, 1950; Sidman, 1957; Solomon and Wynne, 1953). In most experiments on the acquisition of avoidance responding, the subject is also allowed to escape from shock when it fails to avoid, and on these trials, shock termination reinforcement seems to influence the probability of avoidance. Church and Solomon (1956), for example, have shown that fewer animals learn to avoid when there is a delay in shock termination following a response during shock.

The present experiments were designed to investigate the acquisition of an avoidance response when reinforcement of the response by shock termination was prevented. It was hoped that the results of this research would reveal the relative importance of this variable in controlling avoidance learning.

EXPERIMENT I

The effects of preventing reinforcement of the response by shock termination were studied in two experiments. In one, free responding during the inter-trial interval was permitted; in the other it was not. Since the apparatus and procedures of these experiments were almost the same, they have been combined in a single analysis.

METHOD

Subjects

The subjects were 24 experimentally naïve mongrel dogs.

Apparatus

The apparatus was a modified Miller-Mowrer shuttle-box. The avoidance response was that of jumping from one compartment of the shuttle-box to the other. The US

[1]This research was supported by a grant to Dr. R. L. Solomon from the Medical Sciences Division of the Rockefeller Foundation. It was facilitated by the Laboratory of Social Relations, Harvard University. The authors would like to thank Dr. R. L. Solomon for his assistance and advice. Mr. J. Kirman and Mrs. W. Morse assisted in the performance of part of Experiment I.

[2]USPHS Post-doctoral Fellow.

was an intense electric shock (4.5 ma.) delivered to the dog through the grid floors of the apparatus.

In the experiment in which inter-trial responses were not permitted, the CS was that of dimming the illumination in the dog's compartment of the shuttle-box, and simultaneously raising the gate between compartments. For the dogs in the experiment in which inter-trial responses were permitted, the CS was a tone.

Design and Procedure

There were four experimental groups with six dogs in each group. These can be conceived of as forming a simple 2×2 factorial design. The groups were: the NR-NE group, in which dogs were not permitted to respond during the inter-trial interval and were prevented from escaping shock; the NR-E group, in which dogs were not permitted to respond during the inter-trial interval but were allowed to escape shock; the R-NE group, in which dogs were permitted to respond during the inter-trial interval and prevented from escaping shock; and the R-E group, in which dogs were permitted to respond during the inter-trial interval and also were allowed to escape shock.

All dogs were first exposed to ten CS presentations of 2 min. duration. Dogs that jumped during these CS presentations were discarded.

Each dog was then subjected to 50 acquisition trials, 10 trials a day. On each trial, if the dog jumped during the 10 sec. CS-US interval, it avoided shock and terminated the CS. If the dog failed to jump during the CS-US interval, the shock was turned on 10 sec. after CS onset. The four groups differed in treatment following shock onset.

For the dogs in the NR-NE and R-NE groups, the gate between compartments of the shuttle-box was lowered at shock onset. Thus these dogs could not jump to escape shock and reinforcement of the jumping response by shock termination was prevented. On these shock trials, the inescapable shock lasted for 5 sec., at which time both shock and CS were terminated. For dogs in the NR-NE group, the gate between compartments of the shuttle-box remained closed until the beginning of the next trial, thus preventing inter-trial interval responses. For dogs in the R-NE group, the gate between compartments was opened immediately following shock termination, and thus inter-trial interval responses could occur.

For the dogs in the NR-E and R-E groups, the gate between compartments of the shuttle-box was left open at shock onset. The shock remained on until the dog jumped at which time both CS and shock were terminated. Thus reinforcement of the jumping response by shock termination could occur. For dogs in the NR-E group, the gate between compartments of the shuttle-box was lowered after the dog had escaped, in order to prevent inter-trial interval responses. For dogs in the R-E group the gate was left open in order to allow inter-trial interval responses.

Thus, while all dogs could avoid shock by jumping during the CS-US interval, only dogs in the NR-E, and R-E groups could escape the shock by jumping after its onset.

Results

The average number of avoidances for blocks of ten trials are shown in Figure 1. Dogs in both groups in which escape from shock was permitted (NR-E and R-E), learned more quickly and reached a higher level of responding than dogs which could not escape shock.

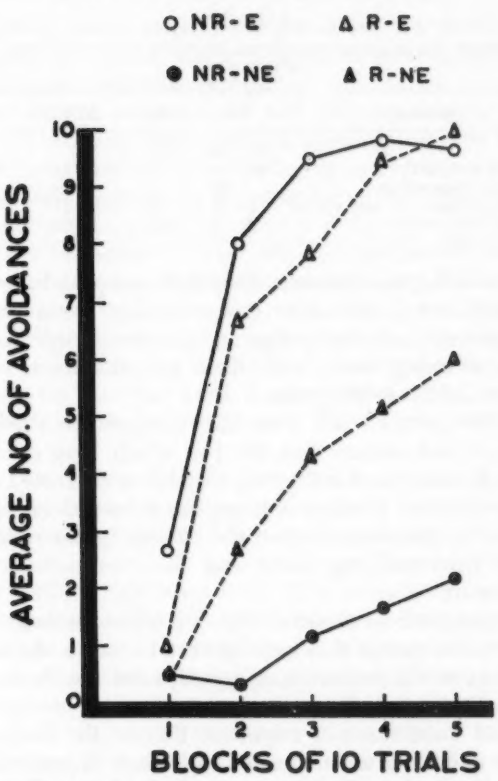

FIGURE 1. Average number of avoidances for each block of ten trials. See text for explanation of group designations.

The total number of avoidances made during the fifty acquisition trials was subjected to an analysis of variance shown in Table I. A 2×2 factorial design was used, and the data were treated using the Freeman-Tukey square root transformation. The main effect in which escape *versus* no escape was compared was significant at the .05 level. The other main effect, comparing inter-trial interval responding *versus* no inter-trial interval responding, was not significant. The probability of the F for the interaction effect lies between .10 and .05.

In order to compare the relative positions of the four groups at the end of acquisition, the number of avoidances on the last 20 trials was analysed using the Method of Allowances (Mosteller and Bush, 1954).

TABLE I

ANALYSIS OF VARIANCE OF THE TOTAL NUMBER OF AVOIDANCES DURING ACQUISITION

Source	SS	df	MS	*F*
Total	260.41	23		
Escape treatment	147.51	1	147.51	31.79
Inter-trial responding	1.98	1	1.98	<1
Escape × inter-trial responding	18.21	1	18.21	3.92
Residual	92.71	20	4.64	

There was no difference between the NR-E group and the R-E group (both of which were allowed to escape shock). However, there is a difference between these two groups and the two groups that were prevented from escaping shock. Also there is a difference between the NR-NE group and the R-NE groups.

Thus, the two groups which were allowed to escape shock reached a higher level of performance than the two which were prevented from escaping shock. Also, the R-NE group, which was prevented from escaping shock but allowed to respond during the inter-trial interval, reached a higher level of performance than the NR-NE group which was prevented both from escaping shock and from responding during the inter-trial interval.

While the group curves shown in Figure 1 reflect the individual curves for dogs in the two groups that were allowed to escape shock, the group curves for dogs in the two no escape groups did not do so. When the data for the 12 dogs receiving the latter treatment are combined, we find a bimodal distribution of responses. Five of the twelve gradually increased the number of avoidances on each block of ten trials (three at the same rate and two slightly more slowly than control group dogs). The other seven made very few avoidances throughout the 50 training trials. For the first five animals the average number of avoidances was 28 and the range from 14 to 40; for the other seven, the average number of avoidances was 1 and the range 0 to 4.

DISCUSSION

Solomon and Wynne (1953) state that the reinforcement of an escape response by shock termination will increase the probability of that response recurring in the presence of the CS. According to such a theory the effect of shock termination reinforcement would be particularly important during the early stages of acquisition (before any avoidances have been reinforced by CS termination) since the reinforcement of escapes would act to shape up the avoidance response, that is, to increase the probability of avoidances before they occur as such. Thus, when

reinforcement of escape responses is prevented, we would expect very few avoidances if, in fact, shock termination reinforcement is a main factor affecting the avoidance response.

While our data in general agree with this explanation, the bimodal distribution of responses among non-escape dogs suggests that, at least for some animals, an alternative shaping-up mechanism was effective in producing enough responses in the presence of the CS so that avoidance learning could occur. A number of such alternatives can be suggested. One is the reinforcement of "superstitious" responses by shock termination. If, for example, the dog had been approaching the barrier between a compartment of the shuttle-box when shock was terminated, the probability of this response would be increased the next time the CS was presented. This, presumably, would have facilitated avoidance learning on later trials. On the other hand, if some "superstitious" response which interfered with jumping was reinforced, the opposite effect would have occurred.

One can also suggest that whether an animal responds to the CS or not will simply depend on its operant jumping level. Those dogs that have a higher level will respond in the presence of the CS, be reinforced by CS termination, and then go on to learn. Those which have a low operant level will simply never jump often enough for reinforcement by CS termination to have an effect. All animals in these experiments were selected for low operant jumping level since dogs which jumped during the ten pre-test trials were discarded. Therefore, it would seem that this hypothesis is not applicable unless one amends it to state that the operant jumping level in an aversive or fear-provoking situation is the important variable, not simply operant jumping level alone. While the dogs may have begun with the same low operant jumping levels, the aversive US may have raised the response probability differentially.

While the results discussed above would be expected on the basis of most theories of avoidance learning, there is one finding which would not. The prevention of escape responses had a less pronounced effect on avoidance learning when inter-trial interval responding was allowed to occur. While a number of explanations for this effect can be suggested, none seems particularly plausible and, therefore, it would seem that an adequate explanation of this phenomenon must await the isolation of further experimental variables.

Experiment II

In this experiment an attempt was made to isolate further the effect of the non-escape procedure on dogs which failed to learn to avoid. This was done by subjecting these dogs to the usual avoidance learning pro-

cedure after the 50 non-escape training trials. It was assumed that the influence of this prior experience on behaviour would be revealed in transfer effects.

Method

Subjects

The subjects were five dogs in the NR-NE group which failed to avoid shock when escapes were prevented. An additional dog was run under this procedure and failed to avoid. It was added to this group bringing the number to six. These dogs made a total of five avoidances out of 300 possible. They were compared with the six dogs in the NR-E group.

Apparatus

The same apparatus was used in this experiment as in the previous one.

Procedure

Following completion of the procedures described in the previous section the dogs in the NR-NE group were switched to the usual avoidance learning procedure in which they could escape shock. Each animal was run until it met the acquisition criterion of 10 consecutive avoidances on a given day.

The six dogs in the NR-E group, which were run for 50 trials employing a procedure which allowed them to escape, were continued on this procedure until they met the acquisition criterion, if they had not already done so. Thus a comparison was made between experimental group dogs which had been exposed to a series of paired presentations of CS and US before avoidance training with escape, and control group dogs which began avoidance training without prior exposure to CS and US.

Results

Table II presents indices of acquisition of the avoidance response for the dogs in the prior exposure group after they had been switched to the procedure allowing them to escape shock, and for the animals in the control group which had received no prior exposure to CS and US. The

TABLE II

Indices of Acquisition of the Avoidance Response for Dogs in the Experimental and Control Groups

Indices	Experimental group	Control group
Trial of last shock		
Median	11	14
Range	9–23	7–41
Trial of first escape		
Median	5	1
Range	2–16	1–1
Difference between trial of last shock and trial of first escape		
Median	6.5	10
Range	5–8	6–40

trial of the last shock gives an over-all index of speed of acquisition. There was no difference between groups in trial of last shock (Mann-Whitney U test: $P\ (U < 16) = .409$). However, there were differences between groups in the sequence of behaviour leading up to the final acquisition of the avoidance response. The dogs in the prior exposure group required more trials to make the first escape response, and were exposed to shock longer than dogs in the control group (Mann-Whitney U test: $P\ (U = 0) = .001$). The median number of seconds of shock before the first escape was 503 for dogs in the prior exposure group, and 20 for dogs in the control group. Furthermore, once the prior exposure group subjects had made the first escape response, they showed somewhat faster learning than control group animals. The number of trials between the first escape and the last shock was less in the prior exposure group than the control group (Mann-Whitney U test: $P\ (U < 5.5) = .053$).

Discussion

The prior pairing of CS and US had two effects on later avoidance learning. The most striking was the greater number of trials before the occurrence of the first escape. These data support the hypothesis that some "superstitious" response was reinforced by shock termination during the 50 trials when escapes were prevented. Presumably these "superstitious" responses were incompatible with the proper avoidance response among those dogs which failed to learn when escapes were prevented. When these dogs were switched to a procedure permitting escapes, this "superstitious" response also interfered with the acquisition of escape responses.

The second transfer effect observed was faster avoidance learning following the trial of first escape among those dogs which had received prior pairing of CS and US. For these dogs it would seem that the properties of the CS as a discriminative and aversive stimulus were better established than for the dogs which had received no prior exposure to CS and US. Once the correct response was emitted in the presence of the CS, avoidance learning would occur more rapidly.

In theories of avoidance learning the pairing of CS and US had been assumed to result in the CS acquiring aversive or fear-provoking properties which facilitate avoidance learning. While the results of this experiment agree with this view, they also indicate that this procedure results in the reinforcement by shock termination of particular responses which may or may not facilitate avoidance learning. If an inappropriate response has been learned in aversive situations, the early stages of avoidance learning may proceed more slowly.

Summary

Dogs were trained to avoid an intense electric shock by jumping from one compartment of a shuttle-box to the other. The experimental group could avoid shock but could not escape shock during acquisition; the control group could both escape and avoid shock during acquisition. Avoidance learning was significantly better in the control group than in the experimental group. When inter-trial interval responding was allowed, dogs in the experimental group made more avoidances during the final stages of training than when it was not.

Animals that failed to learn to avoid in the experimental group were then switched to the procedure which allowed them both to escape and avoid shock. Two effects were noted in comparing these dogs with dogs that had been trained on the escape-avoid procedure from the very beginning. Animals which had previously been subjected to pairings of CS and US required more trials before they made their first escape from shock, and showed faster avoidance learning after they made their first escape.

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A TECHNIQUE OF INTERMITTENT STIMULATION FOR MEASUREMENT OF TACTUAL SENSITIVITY: APPARATUS AND PRELIMINARY RESULTS[1]

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THE PURPOSE OF THIS PAPER is to describe a method of investigating the discriminatory or resolving power of the skin by means of a "flicker" technique analogous to that employed for a great many years in vision in the measurement of the critical flicker frequency (c.f.f.). This technique consists of producing an interrupted stream of air at a specified pressure whose frequency can be systematically increased until the subject reports a constant sensation of pressure on some specified part of the skin. The frequency at which this constant sensation occurs is referred to as the *critical frequency of percussion* (*c.f.p.*)—the name first given this phenomenon some 35 years ago by Allen and Hollenberg (1924). The apparatus was originally devised for use in some gerontological research on the relation of changes in tactual sensitivity to a numerical loss in skin receptors with age, that has been shown to occur in various areas of the body. For this research any one of the several traditional methods for studying tactual sensitivity could have been used, for example, two-point threshold, graded series of stimulus hairs, or electrical stimulations, but for various theoretical reasons it was felt that a technique employing some form of intermittent stimulation would prove to be more fruitful and would provide a better measure of the resolving power of the skin. Since the apparatus has proved to be extremely reliable, it is our purpose here to describe its nature and its use in the measurement of the tactual sensitivity of the finger-tips.

The flicker technique, as is well known, is a most useful method of studying visual processes. Only twice, however, has it been used in any systematic fashion in the study of tactual perception, and these studies were largely of an exploratory nature involving only a few subjects. These investigations were carried out by Allen and Hollenberg in 1924 and by Bellows (1936). In both studies the intermittent stimulation was produced by means of a sectored disk which was rotated in front of a nozzle through which air was forced at a specified pressure. This apparatus, while having the merit of simplicity, yielded consistent and reliable

[1]This research was supported by a grant-in-aid from the Associate Committee on Applied Psychology of the National Research Council, Canada.

readings only after a considerable amount of practice. Allen and Hollenberg report that at least a week of daily practice sessions of several hours' duration were required before any kind of consistent readings could be obtained. Similarly, Bellows reports the necessity of practice sessions of up to five weeks' duration and even then some of the subjects could not be used because of excessive variability in their scores. The apparatus described here has the virtue of producing consistent readings after only a few practice trials.

Method

Apparatus

A diagram of the tactile stimulator together with the various components that make it up is shown in Figure 1. It consists of a large compressor tank which can be filled

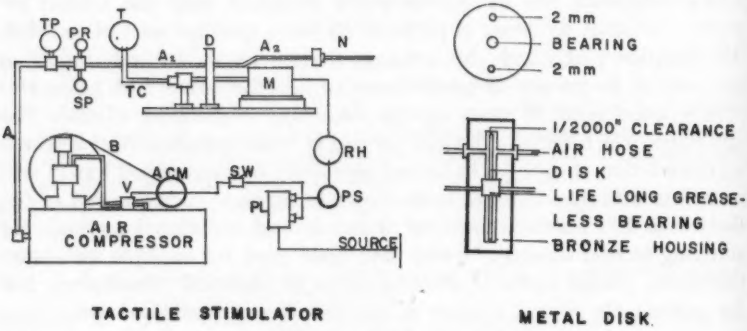

Figure 1. Diagram of the tactile stimulator.

A_1	Copper air hose, diameter 7 mm.
A_2	Copper air hose, diameter 2 mm.
D	Metal disk in bronze air-tight housing
ACM	Air compressor motor
B	Compressor belt
PR	Pressure regulator (stimulus pressure)
PS	Powerstat
PL	Wall-type plug
M	Motor (electric variable speed)
N	Nozzle
RH	Rheostat
T	Tachometer (measuring r.p.m. in hundreds)
TC	Tachometer cable
V	Air compressor valve
SW	Switch
SP	Pressure gauge (stimulus pressure)
TP	Pressure gauge (tank pressure)

up with air to a maximum load of 100 lb./sq. in. A copper tube (A_1) leads from the tank through a tank pressure gauge (TP) to a stimulus pressure regulator (PR) by means of which the pressure of the outgoing air can be set at any value from 1 to 60 lb./sq. in. This value can be read from a pressure gauge (SP). Since the stimulus pressure values which are used are always well below the load of 100 lb./sq. in. in the compressor tank, many stimulus trials can be given before the compressor tank has to be filled again to its maximum load. From the pressure regulator a second copper tube (A_2) conducts the air to a bronze air-tight casing enclosing a rotating metal disk (D) with two openings which interrupt the incoming stream of air.[2] From the disk casing, the air continues in bursts along a tube to a coupling on the outside of the apparatus to which nozzles of various diameters can be attached. The metal disk is rotated by a small electric motor (M) whose speed is controlled by a powerstat (PS) and rheostat (RH) joined in series. A mechanical tachometer (T) is connected to the motor to record the speed of the rotating disk in revolutions per minute. The r.p.m. readings can then be changed in bursts per second by using a conversion table.

Preliminary Procedure

The experimental routine in the exploratory stage of the project was as follows: Ss were fitted with earplugs and N.R.C.-type earmuffs in order to eliminate the noise of the apparatus. A stand was erected to hold the left index finger steady at a distance of 0.50 cm. from the nozzle having a diameter of 0.60 mm. A thin film of petrolatum was spread on the finger-tip to protect it from the drying action of the bursts of air. The tank was then filled to its maximum load of 100 lb./sq. in, and the stimulus pressure gauge set at some specified value below 60 lb./sq. in. (the highest reading on the dial). The burst frequency was then gradually increased until S reported a constant pressure sensation. The stimulus pressure was then set at another value and the burst frequency again increased until fusion was reported. It was soon observed that some of the Ss had difficulty in sensing the bursts at pressures below 10 lb./sq. in. Consequently, all subsequent measurements were confined to a range from 10 to 60 lb. at intervals of 5 lb. After various preliminary observations were made, four graduate students were used, each of whom was given 15 trials on each of the 11 pressure values. These trials were spread over intervals of from 10 sec. to 2 days. Increasing, decreasing, and randomized orders of presentation were tried. An analysis of the data showed that the variability in readings averaged 4.2 bursts per second over both short and long periods of time. Furthermore, it was found that reliable readings could be obtained after as few as five practice trials. An analysis was also made to determine possible order effects in pressure presentation. No significant differences in readings were found for the different orders employed.

The final preliminary observations were concerned with attempting to verify some interesting results reported by Allen and Hollenberg (1924). These investigators reported the existence of two fusion points for the finger-tip for every stimulus pressure used. They described this phenomenon as follows: "It was now quite apparent that there are two points of fusion of interrupted stimuli. This was verified by starting the rotation of the disk very slowly at some pressure of the air until one

[2]The air bursts which originate as sharp, round pulses are distorted somewhat by passage along the tube and by their impact with the outside air. Although the air bursts still feel very discrete to S, their physical form is undoubtedly a complex one whose exact nature would be difficult to determine.

fusion point was reached. The speed was then increased and the sensation again became interrupted. On further increasing the speed a second point was reached where there was apparent fusion of the stimuli. This second fusion point is not of the same nature as the first; it was less satisfying, so to speak, but still it was a definitely measurable point of fusion."

They attributed the first fusion point to superficial skin sensitivity and the second fusion point to deep sensitivity. When this procedure was repeated in the present case all four *Ss*, in addition to the two *Es*, reported the existence of two fusion points with 2 of the six reporting the second fusion point to be more "satisfying" than the first. The increment in frequency required to produce the second experience of fusion averaged about eight bursts per second above the first. These two fusion points were also reported when the frequencies were presented in a descending order, that is, from high to low frequencies. Six other *Ss* were then used. Five of these consistently reported both fusion points while one experienced a second fusion only occasionally.

The procedure was next repeated on the skin of the forearm. Of the four *Ss* used, three did not report the second experience of fusion while the fourth experienced it occasionally. These conflicting results prompted a close check of procedure and apparatus which resulted in no hint as to the possible reason for the results until it was decided to test the *Ss* in another room away from the compresser tank, using a connecting hose 4 ft. long and 5 mm. diameter. The finger was again placed at a distance of 0.50 cm. from the nozzle. Owing to pressure loss in the connecting hose, it was impossible to take readings under 20 lb./sq. in. With this experimental arrangement, all 12 of the *Ss* used reported the presence of one fusion point only, regardless of the body locus stimulated. Furthermore, the trial to trial variations in c.f.p. were less than with the previous set-up: the differences between trials, after very little practice, averaged 1.6 bursts per sec. Thus it appears that the two fusion points reported by Allen and Hollenberg (1924) were probably due to some artifact of their procedure, possibly to vibration produced by the machine. This finding is in keeping with that of Bellows (1936) who in commenting on the second fusion point stated that "we did not find it definitely measurable." Unfortunately Bellows does not elaborate on this statement and, consequently, it is not clear whether his *Ss* did not experience the phenomenon or whether their second fusion scores were too variable to suit *E*.

Experimental Procedure

Ten university students, five males and five females, were used in the experiment. They were fitted with earplugs and N.R.C.-type earmuffs and seated at a table in a room adjoining the tactile stimulator. The volar aspect of the terminal phalanx of the left index finger was placed in a stand at a distance of 0.50 cm. from the nozzle of 0.60 mm. diameter. A thin film of petrolatum was applied to the finger-tip. Each *S* was given one practice trial on each of the nine stimulus pressures ranging from 20 to 60 lb. at intervals of 5 lb./sq. in. Each trial consisted of increasing the frequency of the air bursts until *S* reported a constant or fused sensation. This value was called the c.f.p. or critical frequency of percussion. Following these practice trials each *S* was given one trial, separated by a 10 sec. rest period at each of the nine pressures. This was then followed by a rest period of a minimum of 6 min. This procedure was repeated until five trials had been given at each of the pressures. On the following day five more trials were given resulting in a total of ten trials for each *S* at each of the nine stimulus pressures.

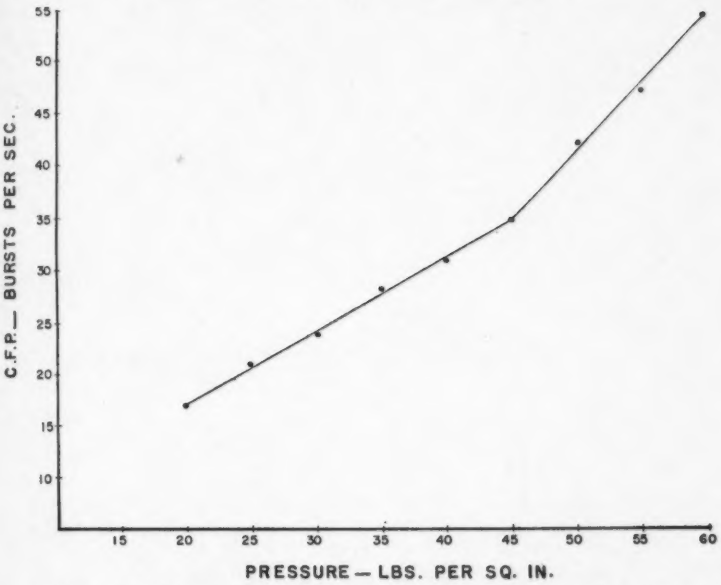

FIGURE 2. Relation between c.f.p. and stimulus pressure. Each point is the mean of 100 measurements, 10 from each of 10 subjects.

RESULTS AND DISCUSSION

The main findings of the investigation are summarized in Figure 2 where critical frequency of percussion (c.f.p.) is plotted against stimulus pressure. It can be seen that the graph consists of two linear parts or two limbs. When the c.f.p. was plotted against the logarithm of pressure, it was found that the data conformed to the Fechner logarithmic law of response to stimulation: $N = k \log P + b$, where N represents the c.f.p., P is the stimulus pressure, and k and b are constants which differ in numerical value for the two limbs of the graph. The sudden change of slope of the first linear part of the graph at 45 lb./sq. in. is most interesting for it suggests the activation of some other sensory system. That this may be so is suggested by the research on visual flicker where the graph showing the relation between c.f.f. and changes in light intensity is also made up of two limbs. In this case the lower limb is believed to be due to rod action and the upper limb to the action of the cone receptors (Crozier & Wolf, 1941). In the present experiment we believe that the lower limb represents light pressure sensitivity and the upper limb, deep

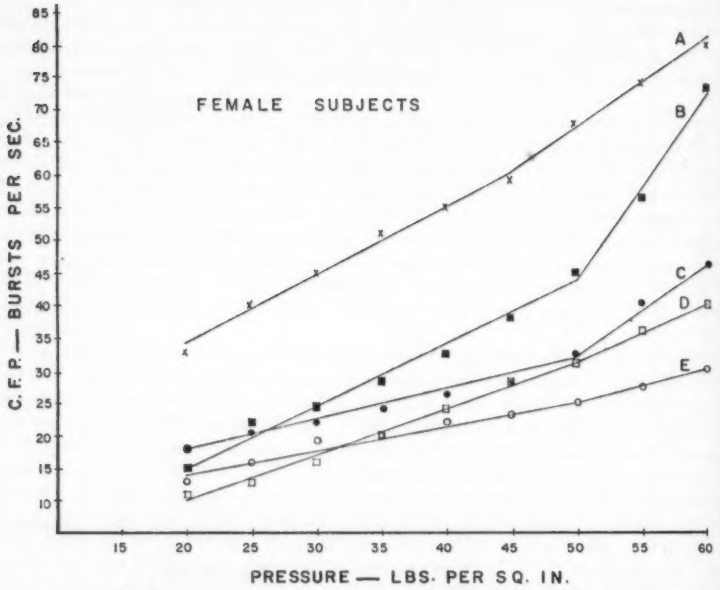

FIGURE 3. Relation between c.f.p. and stimulus pressure for five female subjects (A, B, C, D, E). Each point is the mean of ten measurements.

pressure sensitivity. One possible way to test this hypothesis would be to apply to the finger-tip nupercaine or some similar substance known to inactivate the superficial skin reactors yet leave the deeper receptors relatively intact; then repeat the experiment. Such an experiment will shortly be performed.

Figures 3 and 4 show the individual performances of the ten subjects, five males and five females. One of the main reasons for plotting separate graphs was to show the large individual differences which exist in the discriminatory or resolving power of the skin. At 20 lb. pressure the c.f.p. values for the female subjects range from 10 to 33 bursts per second and for the males from 5 to 27. These differences are even more pronounced at the higher pressure levels. For example, at 60 lb. the c.f.p. values for the females range from 30 to 80 bursts per second and for the males from 25 to 75. No significant sex differences are present ($t = 1.01$, $P > .10 < .20$). In addition to the differences in c.f.p. there are also large variations in the slope of the second limb of the curve. For example, curves B, C, F, and G show a steep upward slope while curves A, D, E, and J show only

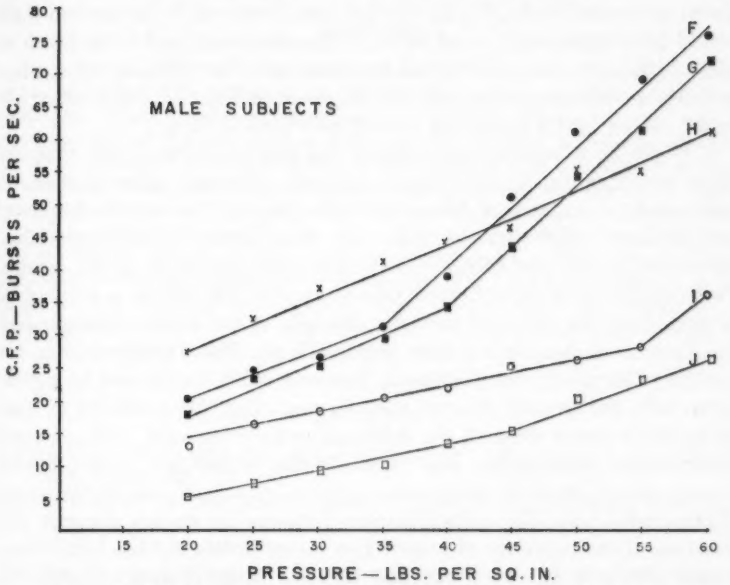

FIGURE 4. Relation between c.f.p. and stimulus pressure for five male subjects (F, G, H, I, J). Each point is the mean of ten measurements.

a slight upward slope. Since these last four curves show only a slight indication of a second limb, one may raise the question as to whether a second limb actually exists. That it does exist is indicated by two factors. First, when each of these curves, which represent the mean of ten readings at each pressure, is reduced to ten curves each representing one reading at each pressure, at least 80 per cent of the curves show a small but nevertheless noticeable change in the slope of first linear part of the curve at the higher pressures. The second factor is the small trial to trial variation in scores which averages 1.6 bursts per second. With this degree of consistency of measurements, even a small deviation in the slope of a curve can be considered to be of practical significance. These factors, taken in conjunction with the results of Allen and Hollenberg (1924) and Bellows (1936), who also found two limbs, confirm our belief that the curve showing the relationship between c.f.p. and pressure is composed of two limbs. The slope of the second limb, however, varies from subject to subject. Finally, there are also individual differences as to the pressure at which the second limb appears. For example, one subject shows the second limb at 35 lb. (F), three at 50 lb. (B, C, D), and one

shows no second limb (H). In this last case, presumably the second limb would have appeared beyond 60 lb. if measurements had been taken at these higher pressures. It should be mentioned that although the inter-individual differences are marked the intra-individual differences are small, averaging 1.6 bursts per second over several days.

It would be worthwhile to compare the present findings with those of other investigators. Unfortunately, however, the only other systematic work on the c.f.p., that of Allen and Hollenberg (1924) on the finger-tip and Bellows (1936) on the lower lip, was largely of an exploratory nature employing two subjects in the first case and three in the second. Furthermore, their experimental procedure was different in a number of respects from the one used here, for example, in the nozzle diameter and the distance of skin surface from nozzle. Thus a direct comparison is not possible. Despite these differences, however, both studies are in agreement with the present investigation in reporting the existence of two limbs in the curve showing the relationship between c.f.p. and pressure. Furthermore, both studies, but especially that of Bellows (1936), report considerable individual differences in the c.f.p. values and in the slope of the second limb. The main difference between the studies concerns the question of the existence of one or two fusion points. As has been mentioned above, it is very likely that the two fusion points reported by Allen and Hollenberg were due to an artifact and only one genuine fusion point can be experienced.

The tactile stimulator which has been devised has proved easy to use and is capable of providing consistent readings after only a few practice trials. In this respect the apparatus is a considerable improvement over that employed by Allen and Hollenberg and Bellows who required several weeks of daily practice before consistent readings could be obtained, and even then certain subjects could not be used. One possible reason for the poor reliability of their measurements may lie in their use of a rotating sectored disk placed in the immediate vicinity of the skin surface. With this type of arrangement, the whirling motion of the disk would produce air currents in the vicinity of the skin area and thus affect the reliability of the measurements. In the present experiment, this variable was eliminated by placing the rotating disk inside the apparatus. A second possible reason is the vibration of their apparatus which was eliminated here by conducting the tests in an adjoining room.

Although the tactile stimulator was designed specifically for investigating the sensitivity of the finger-tip, it can be used to study almost any area of the skin. Furthermore, experiments analogous to those carried out on visual flicker and its relation to various variables could easily be performed.

Summary

A description is given of the use of a "flicker" technique in the measurement of tactual sensitivity. This technique consists of producing at a specified pressure an interrupted stream of air whose frequency can be systematically increased until *S* reports a constant or fused sensation of pressure on some part of the skin. The frequency at which this constant sensation occurs is referred to as the critical frequency of percussion or c.f.p. Preliminary research with the apparatus indicates that it is easy to use and is capable of producing very consistent readings after few practice trials.

In an experiment to determine the c.f.p. of the finger-tip in ten *S*s it was found that the relation between c.f.p. and pressure consisted of two linear parts and conformed to the Fechner logarithmic law of response: $N = k \log P + b$, where N is the c.f.p., P is the stimulus pressure and k and b are constants which differ in numerical value for the two limbs of the curve. Individual differences were large though the individuals reproduced their c.f.p. values with marked consistency.

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PERSONAL VALUES AND RESPONSE STRENGTH OF VALUE-RELATED WORDS AS MEASURED IN A PSEUDO-PERCEPTUAL TASK[1]

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THERE HAS RECENTLY been considerable experimental interest in the determinants of recognition thresholds for words which differ in their frequency of occurrence in the language or in their motivational and emotional significance to subjects. In experiments (Postman, Bruner, & McGinnies, 1948; Postman & Schneider, 1951; Solomon & Howes, 1951) where the words have been chosen so as to be related to the different value areas on the Allport-Vernon Study of Values, it seems satisfactorily established that both word frequency and personal values act as determinants with word frequency as the major determinant.

A question of current interest is how these non-perceptual variables have their effect on the threshold. One possibility is that the effect is limited to the verbal response the subject makes to the stimulus (cf. Solomon & Howes, 1951). The second possibility is that the non-perceptual variables interact with the perceptual process in such a way as to sensitize it to relevant stimuli (cf. Postman, 1953; Postman, Bruner, & McGinnies, 1948).

In a recently reported experiment, Goldiamond and Hawkins (1958) obtained results relevant to this question. In a training task they controlled the frequency with which subjects saw and pronounced nonsense syllables. In a pseudo-perceptual task which followed, the subject was instructed that on any given trial one of the syllables which he had seen would be presented for a brief interval and his task was to guess which of the syllables it was. Instead of a syllable being exposed, however, Rorschach Plate 1 was flashed for .02 sec. Under these conditions Goldiamond and Hawkins obtained the logarithmic recognition-frequency curve of an actual threshold experiment. As they point out, these results must be interpreted entirely in terms of a response bias created by experimentally controlled word frequency.

The object of the present experiment was to investigate the effect of personal values on the response bias or strength of value-related words employing the Goldiamond and Hawkins procedure.

[1]The research for this paper was supported (in part) by the Defence Research Board of Canada under Grant no. 9401–13. My thanks are due to Miss Janet Hay for experimental assistance.

METHOD

Subjects

Subjects were 18 male undergraduate students enrolled in an introductory psychology course. They had taken the Allport-Vernon-Lindzey (1951) Study of Values in class and were selected on the basis of the clear dominance of one of the six values in their profile. The selection was such that there were three Ss whose highest score was in the theoretical area, three whose highest score was in the economic area, etc. The mean age of the Ss was 21 years with a range of 19 to 25 years (to nearest birthday).

Apparatus

Twelve words, two for each of the six value areas were chosen from the longer list used by Postman, Bruner and McGinnies (1948). The words and their frequency of occurrence are given in Table I. Each of the words was typed in upper-case

TABLE I

STIMULUS WORDS

Theoretical	Economic	Aesthetic	Social	Political	Religious
Analysis (14)*	Useful (50)	Beauty (100)	Friendly (50)	Politics (40)	Blessed (6)
Research (22)	Wealthy (27)	Poetry (26)	Helpful (14)	Dominate (11)	Sacred (38)

(Mean log frequency = 1.41)

*Frequency of occurrence per million according to the Thorndike-Lorge general count.

letters in the centre of ten 3 × 5 in. cards, making up a deck of 120 cards in which each of the 12 words occurred with the same frequency.

A Gerbrand's (modified Dodge) tachistoscope was used for the pseudo-perceptual task.

Procedure

The experiment consisted of three parts carried out consecutively in a single session lasting about 40 min. These parts were: a training task, interpolated task, and a pseudo-perceptual task.

Training task. When he came to the experimental room S was seated at a table and given the deck of 120 cards and instructed as follows: "I am going to give you a deck of cards. A word is printed on each. I would like you to look at each card carefully, and then pronounce the word. Continue through the deck, pronouncing each word as you come to it—turn it over when you have finished with it." The order of occurrence of the words in the deck was randomized for each S by shuffling.

Interpolated task. Following this training task S was given a short story to read. The purpose of this task was to reduce the primacy of recall for the last words in the deck. This task took approximately 10 min.

Pseudo-perceptual task. At the end of the interpolated task S was seated before the tachistoscope and given the following instructions: "I am going to present to you, one at a time, the words you pronounced earlier from the deck of cards. If you look in the eyepiece (here) of the box you will see two lines. The words I shall show you will appear directly between the lines. Each word will be presented for a very short period of time and you may not be able to tell what the word is, at first. However, after each presentation I want you to make a guess as to what the word

was. Remember, even if you do not recognize the word, I still want you to tell me what you think it was. Each word will be presented to you several times until you have correctly recognized it. I will inform you when you are correct, and then another word—or the same word, by random selection—will be shown. I shall say 'ready' before each exposure of the word."

On each trial a plain white card was exposed for .02 sec. S's responses were recorded on a data sheet prepared in advance. The sheet had 12 columns and 25 rows, each column being headed by a different one of the 12 words. The order of the words at the top of the columns was randomly determined for each S. When S said the word at the top of the column in which the experimenter was recording, he was told he was correct and the experimenter began recording in the next column. If S failed to say the word at the top of the column before 25 flashes had been presented, he was told that another word would be tried. This procedure was repeated until the experimenter had recorded in all 12 columns of his data sheet. This method of recording provided two ways of terminating a trial: either when S "correctly identified" the word or when 25 flashes had been presented. It should be noted that the method resulted in Ss giving an unequal number of responses.[2]

Results

The main results are summarized in Table II. Since subjects were selected because of the clear dominance of one value regardless of the

TABLE II

Mean Number of Different Words Said and Mean Frequency of Words Said in Dominant and Non-Dominant Value Areas

Ss	Mean no. different words said in dominant value area (Rank 1)	Mean no. different words said in non-dominant value areas (Ranks 2–6)	Mean frequency of words said in dominant value area (Rank 1)	Mean frequency of words said in non-dominant value areas (Ranks 2–6)
1	1.0	0.7	16.0	14.3
2	1.0	0.4	37.0	25.8
3	1.0	0.4	37.0	24.2
4	1.0	0.4	36.0	25.2
5	0.5	0.6	41.0	17.7
6	1.0	0.5	24.0	22.0
7	0.5	0.5	35.0	26.8
8	1.0	0.9	11.0	9.7
9	0.5	0.2	100.0	68.5
10	0.0	0.2	0.0	87.0
11	1.0	0.5	23.5	21.4
12	0.5	0.3	101.0	35.3
13	0.5	0.4	38.0	37.5
14	1.0	0.7	23.5	13.4
15	0.5	0.8	33.0	13.6
16	0.0	0.5	0.0	42.0
17	1.0	0.4	30.5	26.8
18	1.0	1.0	9.5	5.6
	$Z = -2.44^*$ ($p = .007$)		$Z = -2.26^*$ ($p = .0119$)	

*Wilcoxon's matched pairs test using signed ranks.

[2]Goldiamond and Hawkins present a full explanation of the rationale behind this method of recording.

shape of the rest of their profile, data for values ranks 2 to 6 were combined for purposes of the main statistical analysis. As may be seen in the table, 12 of the 18 subjects said a greater mean number of different words in their dominant value area, two said the same mean number, and four said a smaller mean number. For the group, the tendency to say a greater mean number of different words in the dominant value area is significant at the .007 level of confidence. If the last two columns of Table II are compared, it may be seen that 16 of the subjects said the words in their dominant value area more frequently than they said the words in their non-dominant value areas. The remaining two subjects were those who

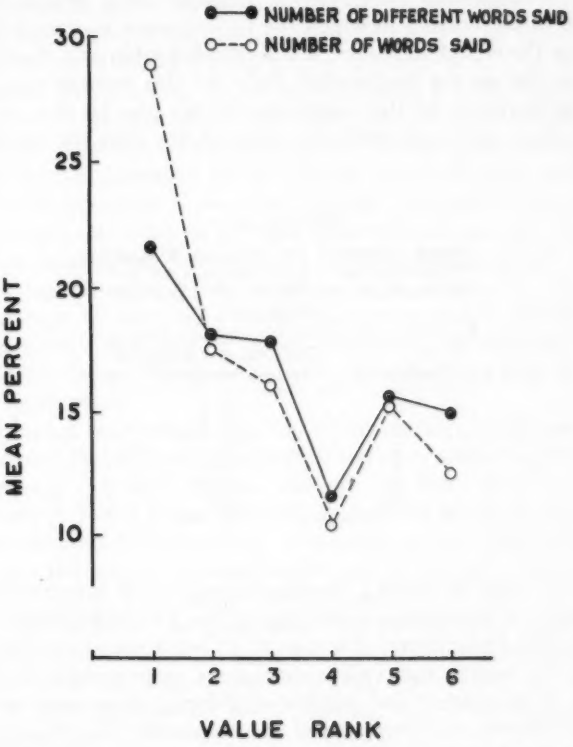

FIGURE 1. Number of different words said in each value area as a percentage of total number of different words said and number of all words said in different value areas as a percentage of total number of words said.

did not say either of the words in their dominant value area. For the group, the tendency to say the words in the dominant value area more frequently than the words in non-dominant values areas, is significant at the .0119 level of confidence.

Figure 1 shows the results for each value rank separately in graphic form. As may be noted from the figure, there is a perfect rank-order correlation between the number of words said in each value rank and the frequency with which these words are said. The degree of association between each of these variables and value rank as measured by Spearman's Rho is .83 ($p < .05$).

Table III presents the mean number of flashes before "recognition" as a function of individual value ranks of stimulus words. Where the word at the top of the column in which the experimenter was recording was not said by the end of 25 flashes, it was decided arbitrarily that it would have been said on the twenty-sixth flash and this number was used in computing the mean for that word. Spearman's Rho for the rank order of these means and rank order by value of the stimulus words is .83 ($p < .05$).

TABLE III

MEAN NUMBER OF FLASHES BEFORE "RECOGNITION" AS A FUNCTION OF INDIVIDUAL VALUE RANKS OF STIMULUS WORDS

Value rank	Mean number of flashes before "recognition"
1	10.4
2	13.3
3	15.0
4	16.0
5	15.2
6	15.7

As may be noted in Table I, the words used in this experiment varied in frequency of occurrence according to the Thorndike-Lorge general count. In order to determine if frequency of occurrence had a significant effect on the results, rank-order correlations were computed between frequency of occurrence and number of different times each word was said by all subjects, and frequency of occurrence and the total number of times each word was said. These correlations are $\rho = -0.20$ (N.S.) and $\rho = -0.18$ (N.S.) respectively. The population frequencies then had no significant effect either on the number of different times a word was said or on the frequency with which it was said.

Discussion

These results show that the response strength of value-related words varies as a function of the individual's values as measured by the Allport-Vernon-Lindzey Study of Values. The three measures of response strength employed all show the same relation to value rank which is strikingly similar to the relation between value rank and recognition threshold as reported by Postman, Bruner, and McGinnies (1948). It becomes a question then as to whether the recognition-value relationship can be interpreted entirely in terms of differences in response strength among value-related words, rendering unnecessary the concept of selective perceptual sensitization.

One important difference between the pseudo-perceptual task and an actual determination of recognition threshold needs to be emphasized in any comparison of results. In the present experiment and in the one by Goldiamond and Hawkins, the subject receives a training session during which the words presented later for "recognition" became familiar. In the pseudo-perceptual task he is instructed that on any given trial one of the words previously presented will be exposed. With the range of responses thus limited, differences in response strength attributable to experimentally controlled frequency or to value connotation may fully account for the results. In an actual determination of recognition threshold, however, the word must be exposed for a sufficient length of time for at least some of its letters to be recognized before the correct verbal response can be made since the subject has no advance knowledge of what words are to be presented. Sensory information must play some part then in eliciting the correct response.

One possible interpretation of the relative roles of response strength and sensory information in determining the recognition threshold is that words having a greater response strength are likely to be said on the recognition of fewer letters than is required for words having a lower response strength. Some evidence in support of this interpretation is to be found in the data of Postman, Bruner, and McGinnies. They found that a large proportion of pre-recognition responses bore a structural similarity to the word actually being exposed. This finding must mean that the subjects recognized some letters of the exposed word before recognizing the word itself, and made responses which incorporated the recognized letters. Further data are required, however, before any firm conclusion seems warranted.

Summary

Eighteen male Ss were selected so that three had their dominant values in a different one of the six value areas on the Allport-Vernon-Lindzey Study of Values.

They were administered a training task in which they pronounced ten times each of 12 words selected so that two were related to each of the six value areas on the Allport-Vernon-Lindzey Scale. In a pseudo-perceptual task Ss were instructed that on any given trial one of the twelve words they had previously pronounced would be presented for a very brief interval and they were to guess at the word presented if they could not see it clearly. Instead of a word actually being flashed, a plain white card was exposed for .02 sec. The results showed a significant tendency for Ss to say more words related to their dominant value area as compared with their non-dominant value areas and to say these words more frequently. These results are interpreted as showing the importance of response strength as a determinant of recognition thresholds.

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A STUDY OF LATENT INFERENCE[1]

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THIS EXPERIMENT PURPORTS to establish the existence of *latent inference learning*. To illustrate, assume a T maze with black and white end-boxes attached, respectively, to the right and left arms. In stage I, animals are run an equal number of times to each of the empty end-boxes. In stage II, the animals find food only in the now detached white end-box, but do not eat. Latent inference consists in learning, by the end of stage II, to run left at the choice point to obtain food though no experience of this novel sequence has ever been permitted.

I know of no unequivocal evidence for latent inference though a study by Tolman and Gleitman (1949), done under different motivational conditions, suggests the existence of such learning. Unfortunately, their study is ambiguous. It may be interpreted as an example of learning to avoid shock (thus establishing latent inference) or as an example of learning to approach food (thereby failing to establish latent inference).

METHOD

Subjects

Ss were 28 naïve, virgin, male, Wistar albino rats about five months old. They were purchased from the Greenacres farm, a local laboratory animal supplier.

Apparatus

The apparatus was a replica of the Tolman-Gleitman (1949) lazy E alley maze, of exactly the same dimensions, differing from its prototype only in the following respects. The two end-arms, and their respective end-boxes, were made discriminably different. The sides of the right end-arm and its end-box were painted black throughout. Their top was covered with a dark piece of cloth (except for a space to permit observation of S in the end-box). Their floor was covered with black, rough sandpaper. In contrast, the sides of the left end-arm and its end-box were painted with 2 in. vertical black and white stripes. Their top was covered with a wire mesh screen porous enough so that there was little, if any, illumination blockage from the light source in the room. The floor of this section of the E maze, both end-arm and end-box, was covered with a soft, dark green, velvet cloth. Both end-arms were so painted that it was impossible for the animals to see the difference between the end-arms

[1]This study was supported by a grant from the General Research Fund of the University of Alberta.

[2]I wish to thank Mr. Park Davidson for his considerable assistance in the present experiment and for his helpful comments on the interpretation of the results.

until they had gone from the choice point to either the intersection at the left end-arm or the intersection at the right end-arm. The rest of the maze, minus the two end-arms, was painted a flat grey on both sides and floor. The wood floor of this section of the maze was left uncovered and its ceiling was constructed of window screen. All of the one-way doors, except for the end-box doors, were painted flat grey on both sides. The end-box doors, on both sides, were painted the same as the sides of their respective end-arms. End-arms were detachable from the maze and their respective end-boxes in turn detachable from them.

During the experiment, a 60-watt light was suspended from the ceiling about 2 ft. directly above the choice point and equidistant from either end-arm. As far as could be determined, the light from this source fell evenly on both sides of the maze. The experimental room was otherwise unlighted and soundproof.

Preliminary Schedule

For two weeks before the experiment, *Ss* were tamed by handling. They were allowed to explore the top of a desk offering only an open field. No preliminary exploration of the maze was permitted. Three days before the experiment, the animals were put in individual cages, not in the experimental room, with water and food (Purina fox chow pellets) always present. On an experimental day, *Ss* were brought to the expeimental room in their home cages, the cages being distributed randomly throughout the room. The position of an individual cage was varied on different days. Following a day's work, *Ss* were returned to the home room.

Training Series

The training series was divided into two stages with the maintenance schedule the same in both stages. An attempt was made to operationally satiate *Ss* for food and water in both training stages after a method utilized by Meehl and MacCorquodale (1948) in a related latent learning study. Essentially, the technique consists in making food and water always available to *S* in the home cage, and, prior to a day's runs, checking *S* in a neutral cage[3] for eating responses over a 5 min. period. In the present study, if *S* showed eating responses during the check period, he was returned to his home cage for 15 min. and then rechecked again, etc. Since *S*'s four trials each day were broken up into sets of twos, separated from each other by at least 30–45 min., *E* took the further precaution of checking the *Ss* for eating responses before each set of runs. *Stage I*: During this period, both end-boxes in the E maze were empty. S was introduced into the maze through a one-way door in the centre arm and given four runs a day for ten days after a method of combined forced and free runs employed by Meehl and MacCorquodale (1948). *S*, on any given day's work, had equal experience of both sides of the maze, from choice point to end-boxes. *S* was never permitted to retrace on any run and was confined in an end-box for 1 min. at completion of a run. Then *S* was removed from the end-box and either returned to his home cage or reintroduced into the maze. At the end of this stage, *Ss* were divided into two groups, A and B. Position habits were also balanced. Position habits were determined by totaling the number of free choices to the left and the number of free choices to the right. *Ss* showing at least 67 per cent choice in favour of either side were designated as preference animals to that side; otherwise, they were called non-preference animals. *Stage II*: To control for possible extra-maze directional influences, the end-arms with their respective end-boxes were detached from the maze and their

[3]The *Ss* were pre-exposed to the neutral cage to prevent fear responses.

positions reversed relative to their original position in the maze; for example, the formerly right end-arm and end-box became the left end-arm and end-box. For half of the Ss (group A), food was now present in the black end-box but not in the white end-box; Ss in group B found the situation reversed. In either case food (Purina fox chow pellets as in their home cages) was distributed in pellet form over the back two-thirds of the appropriate end-box. Further, a small tray of crushed (but dry) fox chow pellets was present at the back end of the appropriate end-box. S had four trials a day to the empty and four to the non-empty end-box for two days. Pilot studies suggested that this programme would be the most likely to result in latent inference learning *if* it was to be found at all. The manner of running follows closely the procedure in the second stage of the Tolman-Gleitman (1949) study. S was inserted into the end-arm to the non-empty end-box, run to that end-box, and confined for 1 min. Then he was inserted into the opposed end-arm with the empty end-box and treated as in the preceding trial. Then he was returned to the home cage. After the other Ss in his group had received the same treatment, S was checked for eating responses and then introduced *directly* into the non-empty end-box and confined for 1 min. Then he was introduced directly into the empty end-box and treated as in the preceding trial. Again he was returned to his home cage to await his next set of two trials. On each day, trials 5 and 6 paralleled trials 1 and 2, and trials 7 and 8 paralleled trials 3 and 4. Thus, the total number of pairings between food and the appropriate end-box for a given S, over the two-day period, was eight; pairings of no-food with the appropriate end-box also equalled eight. It should be noticed that because the animals received eight trials a day in stage II, the Ss were checked for eating responses four times a day. Out of a total of 784 food checks in the experiment, eating behaviour was observed less than ten times.

During stage II, S's behaviour was carefully observed so as to estimate better whether or not he was satiated for food. No S was ever observed to eat food in the maze, or even to pick it up in his teeth. During the early trials in stage II the usual sniffing, turning over of the food with the paws, etc., took place, but during the later trials, the best way to describe S's behaviour was "indifferent." Usually S would enter the box, go to the back end of the box, sniff cracks, etc., and then return to the front of the end-box, engage in cleaning behaviour, and then take a short nap. Ss never showed any "emotional" behaviour in either end-box. Food was so arranged in the box that it was virtually impossible for S not to have some kind of sensory acquaintance with it on every trial. In most cases, Ss actually napped on the food. At the suggestion of Mr. Park Davidson, the running times for the last 12 Ss were recorded. There are four such trials over a two-day period. The latency showed a definite increase (from an average of 6 sec. to an average of 45 sec.) and justified E's impressions of the running behaviour of the first 16 animals during this period. These observations tend to confirm the belief that Ss were satiated for food in stage II.

Test Series

The maze was restored as in stage I and all food removed. Ss were now placed in the maze after 32–34 hrs. without food. With all doors open, Ss were given one free run in the maze. Latent inference was measured by the preponderance of choices into the arm of the maze which led to the intersection with the formerly appropriate food end-arm and end-box. After a great deal of trial and error behaviour at the choice point during the test run, except for three animals (two failures, one successful), when Ss committed themselves, they ran all of the way to the end-box on the side chosen.

Results and Discussion

The results on the test trial show that out of the 14 rats in group B (food in the white end-box), 10 chose correctly; and out of the 14 in group A (food in the black end-box), 11 chose correctly. The appropriate test of significance is to compare the above results with a pure chance expectation of 50 per cent correct response. On the binomial test (Siegel, 1956), the probability of obtaining this result by chance is .02 (one-tail). This result is interpreted as providing evidence for the existence of latent inference learning.

Perhaps the most simple and straightforward explanation of the present results is afforded by the expectancy theory of MacCorquodale and Meehl (1953). The derivation of these results from their theory is easy to construct. I assume the postulates for an expectancy theory available in their semi-formal version of that theory.

Because of the equalized training in stage I, we have, by the mnemonization principle, the equally strong expectations (*a*) $(S_cR_rS_{bb}) = (S_cR_lS_{wb}) > 0$, where S_c is the presence of the choice point, R_r and R_l, respectively, the right and left turns at the choice points, and S_{bb} and S_{wb}, respectively, the presence of the black end-box and the presence of the white end-box. In stage II, because the rats are divided into two groups, the derivation can be restricted to the group finding food in the white end-box (group A) without loss of generality. The proof can be duplicated for the other group (group B). As a result of pairing food with the white end-box and the absence of such pairing with the black box, we have, by the inference principle and (*a*), (*b*) $(S_cR_lS_f) > (S_cR_rS_f) = 0$, where S_f is the presence of food. In both stages I and II, the valences for the end-boxes are assumed to be the same in strength. Also, the valence for food is below eating threshold and, perhaps, zero. This follows from the observed behaviour of the subjects in stage II and the maintenance schedule. Thus we have (*c*) $V_{S_{bb}} = V_{S_{wb}}$ and (*d*) $V_{S_f} < M > 0$, where M designates the threshold for eating response to the valenced object. Thus, at the end of stage II, we have, in accordance with the activation principle, (*a*) and (*c*), (*e*) $S_c{}^ER_l = S_c{}^ER_r$. Also, in accordance with the activation principle, (*b*) and (*d*), (*f*) $S_c{}^ER_l \geqslant S_c{}^ER_r$. At this point, then, it is not possible to predict the subsequently greater choice in favour of the R_l. On the test trial, the rats are 32–34 hrs. hungry. This increases the valence of the food stimulus. That is, (*g*) $V_{S_f} > M > 0$. Therefore, by activation, (*b*) and (*g*), we have (*h*) $S_c{}^ER_l > S_c{}^ER_r$. Now, it is assumed that the law for summation of reaction potentials sharing elicitor and R term obeys some increasing function of its components (MacCorquodale

and Meehl, 1953). Thus, we finally obtain (*i*) $S_c{}^E R_1 > S_c{}^E R_r$ from (*e*), (*f*) and (*h*). (*i*) leads directly to the probability of the correct response.

In the present set-up, absence of eating behaviour may be evidence of a zero drive for food, a drive for food too low to evoke eating behaviour, or a drive for food weaker than a more dominant drive. In the present study, the possibility that the drive for food was superseded by a stronger drive is unlikely. But the possibility that the rats, though hungry, were not hungry enough to eat is feasible. This is the reason why line (*d*) in the above derivation cautiously does not assert that the valence attached to food is zero. The problem is whether the animals, during the training series, were in fact satiated to the extent of zero drive for food. Both the sort of maintenance schedule instituted during stages I and II and the fact of some eating during the checking periods permit doubt as to the actual drive state of the rats in stage II, even though the behavioural symptoms suggest a state of zero drive. The maintenance schedule in the training series is not conducive to making the animal eat to his fill, but only to eating enough to bring about a brief period of non-eating; the difference is between eating one's fill and merely periodic munching. A better maintenance schedule for the rats would be one of 24 hours of no eating, then one hour of eating for some time prior to the experiment and throughout stages I and II of the experiment.

The possibility that the rats were hungry but not hungry enough to eat in the training series raises an interesting question as to the necessity of putting them on a regimen of food deprivation prior to the test trial. That is, the possibility suggests itself that though, in stage II, they were merely not hungry enough to eat, the reaction potential in favour of the appropriate turn on the test run would be great enough to evoke that response. The obvious test is to rerun the present experiment, but to eliminate the deprivation regimen after stage II in favour of the same maintenance schedule as in the training series.

It should be clear that the above possibilities, even if realized, do not invalidate the present study, but merely show that the deprivation regimen after stage II would not be necessary; it is astonishing enough that positive results are obtained under the present conditions.

C. N. Uhl has suggested another interesting possibility. Even assuming that the rats are satiated to zero drive for food throughout the study (including the test stage), it may happen that they would turn in the direction of food on the test trial because of the sheer attractiveness of the end-box plus food. If this turns out to be true, a revision in expectancy theory's concept of valence (which is now construed as a multiplicative function of "need" strength for the incentive S^* and the cathexis for S^*) would be required.

Summary

Rats, satiated for food, were given equal experience in both halves of the Tolman-Gleitman lazy E alley maze with no food present in either end-box. Then still operationally satiated for food, they experienced food in one end-box and nothing in the other end-box, the end-boxes now being detached from the maze. Finally, in the restored maze, and after 32–34 hrs. without food, they were given one free run with all doors open. Out of 28 animals, 21 ran to the side where they had formerly found food. This phenomenon is called "latent inference learning," and is shown to be derivable from the MacCorquodale-Meehl version of expectancy theory.

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BOOK REVIEWS

Freud: The Mind of the Moralist. By PHILIP RIEFF. Toronto: Macmillan Company of Canada Limited, 1959. Pp. xvi, 397. $6.75.

CET OUVRAGE d'une qualité exceptionnelle nous remet à nouveau en face du paradoxe que ne cesse de présenter l'histoire de la pensée scientifique: une grande erreur fournit souvent l'occasion de découvertes plus importantes et plus nombreuses que des demi-vérités dont personne n'ose plus signaler l'insuffisance. Au moment même où on la voyait s'édifier, puis s'imposer à l'attention de tous les chercheurs, on pouvait déjà aisément s'attendre à trouver dans une large part de l'œuvre de Freud une illustration singulièrement saisissante de ce fait depuis longtemps observé. Mais, un certain recul s'avérait indispensable, avant que tant d'intuitions cliniques, dont on pressentait à peine toute la portée, en viennent à revêtir une signification mieux définie, d'ailleurs nettement indépendante de spéculations théoriques accessoires qui n'avaient pu trouver en elles qu'une justification arbitraire. Sur ce point, l'auteur du présent ouvrage jouissait d'un avantage marqué sur ceux qui l'ont devancé dans cette tentative de réflexion sereine sur les principales implications de la psychologie freudienne. Il apparaît plus facile, aujourd'hui, de se placer au-dessus du parti pris déterminé des premiers adversaires auxquels celle-ci devait se heurter comme de l'enthousiasme fanatique de ses premiers adhérents.

De toute évidence, le présent essai marque un effort pour combler une lacune qui se faisait de plus en plus cruellement sentir. Il y avait déjà trop longtemps que l'on s'attardait à chercher dans une psychologie qui a son point d'attache dans l'inconscient irrationnel une explication exhaustive des attitudes sociales, politiques, esthétiques, morales et religieuses de l'homme. Dans ces divers domaines, les hypothèses avancées par la psychanalyse reposent trop souvent sur des assises plutôt précaires; aussi, grâce à l'ample culture dont il dispose, l'auteur rend-il un service fort appréciable en signalant tous les points où des esprits moins avertis risquent de se fourvoyer en les confondant naïvement avec des présuppositions vraisemblablement acquises. Certes, on ne saurait évoquer, en quelques lignes, tous les aperçus que l'on souhaite retenir de la lecture des exposés si denses, qui constituent la trame des divers chapitres. Mais il faut savoir gré à l'auteur de rappeler aux psychologues, trop enclins à l'oublier, que les processus rationnels n'avortent pas toujours et que ce n'est pas par analogie avec le symptôme que l'on peut alors espérer en élucider le dynamisme.

Comme il l'insinue fort à propos, il est grand temps de repenser la relation que la psychologie est appelée à entretenir avec les autres sciences de l'homme, telle que la sociologie, l'anthropologie culturelle ou l'éthique, en s'affranchissant du contexte évolutioniste désuet, qui embrouille encore notre façon d'envisager la constitution *naturelle* du psychisme humain. Trop nombreux sont ceux qui ne voient, dans la *nature*, qu'un résultat accidentel et provisoire de divers conditionnements historiques au lieu d'un dynamisme essentiel dont l'expression se renouvelle en mettant à profit toutes les ressources disponibles.

Lorsqu'il lui arrive d'effleurer, au passage, certains aspects de l'apport proprement psychologique ou thérapeutique de la psychanalyse, l'auteur ne manque pas de déployer la même rigueur logique qu'à l'ordinaire. Malheureusement, sa lucidité n'est plus du tout la même et une facile ironie dissimule mal le caractère simpliste de ses considérations. Mais, au lieu de lui en tenir rigueur, il serait à souhaiter que sa synthèse critique soit reprise sur ce point par un clinicien de grande classe. En attendant, tous ceux qui ont déjà reçu une sérieuse initiation à la psychologie freudienne tireront un immense profit de la lecture de cet ouvrage, et on ne saurait la leur recommander avec trop d'insistance.

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Child and Juvenile Delinquency. Edited by Ben Karpman. Washington, D.C.: Psychodynamics Monograph Series. 1959. Pp. 364. $10.00.

This book presents a fuller account of a series of symposia on Child and Juvenile Delinquency, organized and chaired by Dr. Ben Karpman, between 1949 and 1953. With one or two notable exceptions, the orientation of the thirty-one contributors is primarily psychoanalytical.

The first three of the five conferences reported consider problems of diagnosis, aetiology, and treatment of psychopathic behaviour. In his opening remarks, Karpman observes: "There is no nosological condition in the entire field of psychiatry in which there is so little agreement, and which is subject to so much misunderstanding and equivocation, as that of psychopathy. . . ." The pertinence of this statement is confirmed by the papers and discussions which follow. This is understandable if it is believed, as stated by one contributor, that the only criteria which seem to have any real validity are those derived from psychodynamics. Validity depends in part on the degree to which such data can be objectively verified; and clinical experience and case histories, which provide the main source of material presented during the symposia, pose one funda-

mental problem: it is difficult to discriminate the facts from opinion about the facts.

By a process of negative rather than positive reasoning, it is generally concluded that the psychopath is characterized by an inability to form close personal relationships; an absence of anxiety and guilt feelings; lack of neurotic features and failure to respond to psychotherapy. Accurate diagnosis is considered by Rabinovitch a necessary precursor to treatment—a view not expressed by a number of the participants who point out that some of the diagnostic subtleties of the cases can only be observed in the therapeutic situation.

Bender's assertion that "Gross deprivation in the first two years of any child's life will result in psychopathic behaviour disorder" is provocative. Is this a scientifically established fact or a hypothesis still to be tested? It is suspected that the logic behind such a conclusion is similar to that which states that "because all juvenile delinquents read comics, reading comics is a cause of delinquency." Considering the psychopath as constitutionally inferior or deviant is said to have impeded progress in understanding and handling delinquents. However, if by constitution is meant ". . . the more or less stable product of the interaction of heredity and environment whilst the organism is developing" (A. Lewis, *Proc. Roy. Soc. Med.* 1935, **29**, 325), this need not be interpreted as negating a therapeutic approach, as many clinicians believe.

On the grounds that psychopathy is not co-extensive with delinquency, the remaining two conferences reported are devoted to a study of the latter. Here the environmentalists amongst the participants feel on firmer ground, believing that causative factors are clear and treatment possible. Kanner sounds a cautionary note by referring to ". . . the futility of seeking a common denominator for all instances of childhood delinquency." There is general accord that unsatisfactory familial situations and deplorable social and economic conditions make greatest contribution to development of delinquency.

The question as to whether delinquency may be at times a healthy response to frustrating, intolerable, and harmful circumstances in the environment, and not necessarily symptomatic of an underlying mental or emotional disorder, gives rise to some lively discussion.

Scant attention is given to methods and techniques of treatment, but reference is made to problems of prediction, foster-home and institutional care, and rehabilitation. In general, the symposia are fruitful in posing problems but not in providing the answers. However, in spite of a tendency to be repetitive and platitudinous, the book constitutes a good reference on delinquency for those working in the field.

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Children in Practice. By JOHN PETERSON. Toronto: Macmillan Company of Canada Limited. 1959. Pp. viii, 227. $4.25.

THE PURPOSE OF Dr. Peterson's concise book is "to assist the medical student to see his child patients within their social context." To achieve his goal, the author draws extensively upon information provided by the related disciplines of psychiatry, psychology, and social work.

In particular, the content is addressed to the family doctor. He is seen as the one person most likely to have intimate contact with the average family during the critical periods in its history. If he is to use this privilege to full advantage, he will need more than purely technical, medical skill: he will need a knowledge of the child in his environment.

The general plan of the book is, accordingly, to present a description of environmental pressures, followed by a discussion of the doctor-child relationship, and concluded by a detailed examination of the specific problems encountered by the physician and the agencies which can be co-ordinated in their solution.

The approach is from the point of view of the social scientist. The physician who is so oriented will be called upon to understand and to attempt to identify "neither bricks nor micro-organisms, but morals and beliefs, axioms and assumptions." The doctor, in other words, must develop and cultivate a feeling for the way of life of his child patients and their families within their own special locales.

To accomplish his task, the socially oriented doctor will first of all need to familiarize himself with the norms of family relationships, habits, morals, etc., and to distinguish these from those of his own background. The doctor must become, as it were, neighbourhood-oriented.

In general, the author's information is thoroughly up-to-date, his style is simplicity itself, and his presentation concise. For this reason, however, the material is often sound without being interesting; the author's own attitudes appear to be conservative rather than adventurous. In this sense, the result is perhaps more in the nature of a good handbook, than a dynamic and challenging textbook.

It is unfortunate that the book is rather scantily indexed. Also, there is a regrettable lack of certain key psychological information, such as a thorough discussion of the use of the modern intelligence test in assessing mental defect. Such detailed information is often sacrificed in favour of a pedestrian summary of the obvious. This is typified by the statement, "The immediate family establishes itself at marriage and it seeks to make a home within a separate dwelling; there the children of the union grow."

In spite of these shortcomings, this would appear to be a much-needed addition to the library of the modern medical student and general practitioner. Psychologically, it is accurate and fully in keeping with the most

recent views on the important topics of child discipline, the key significance of "mothering" in child welfare, and the relevance of the cultural milieu to any understanding of the individual.

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Mental Deficiency: The Changing Outlook. Edited by ANN M. CLARKE *and* A. D. B. CLARKE. London: Methuen. 1958. Pp. xvi, 513. 45*s*.

THE PURPOSE OF *Mental Deficiency: The Changing Outlook* is twofold: to review the contributions of experimental psychologists, clinicians, psychiatrists and others to the understanding, assessment, and treatment of the mentally subnormal; and to urge a particular view concerning the status of high-grade defectives—the view that assessment as a defective should not damn an individual to permanent isolation from society and the opportunity to become a useful citizen.

There are seven contributors to the book, four clinical psychologists, two research psychologists, and a speech therapist. Most of them have a "Maudsley" background, which is reflected in the considerable emphasis placed on the importance of rigorous experimental methods. We are told that this book "presents for the first time a critical and comprehensive review of work both in Britain, America and other countries." While the coverage is very wide, ranging from consideration of recent legislation in Great Britain and the report of the Royal Commission on Mental Illness and Mental Deficiency (1954–7) to problems of the statistical characteristics of tests of intelligence, this is bought at some cost in terms of incisiveness and digestibility. There is a good deal of repetition, and Canadian psychologists will find much of the discussion of the laws relating to mental deficiency in Britain not directly relevant to the problems of their own work. However, the book contains a wealth of material, and should serve as a useful reference volume. Individual chapters in Part II (theoretical problems) are devoted to such topics as criteria and classification, heredity and environment, aetiology, individual differences, learning, brain damage and cerebral palsy. There is an extensive bibliography; it contains 777 references, about half of them to papers published in the decade 1946–55.

As to the second purpose, to put forward a plea for a better understanding of the potentialities of high-grade defectives, accounts are given in Part III (practical problems) of the impressive results obtained by Clarke and Clarke in industrial training for institutionalized defectives, and numerous other experiments in social rehabilitation. The argument for greater efforts on behalf of the mentally deficient rests only partially

on such evidence; other relevant points which are made concern the limitations of the reliability and validity of presently available tests of intelligence, the falsity of the doctrine of I.Q. constancy, the ambiguity of the concept of I.Q. itself, and the fact that the social incompetence of defectives is not solely due to their lack of intelligence—the need for well-designed studies of social maladjustment and the outcome of psychotherapy is pointed out.

On occasion the contributors' enthusiasm leads them to overstate their case. It is all very well to point to the limitations of present tests of intelligence, but surely the following statement (p. 29) is rather misleading: "The reliability of measurement is best established by immediate retest, which usually yields a correlation of 0.9 on a properly standardized and validated test. The personal fluctuations which this represents may involve in some cases considerable change in status from one test to the other. Where a precise borderline is used for any purpose, this fact may have repercussions in the way in which, say, a child is dealt with, when one remembers that on one day he might be just below the borderline, and just above it the next." As a plea against the rigid application of classification by I.Q., this is unobjectionable, but for a "borderline" case few psychologists, in the writer's experience, would care to make a classification solely on the basis of test score. The misleading part of the statement is in the second sentence; on the Stanford-Binet, for instance, with a standard deviation of fifteen points and a reliability over 0.9, the error of measurement would be less than five I.Q. points, which means that the possibility of misclassification by as much as ten points would be very remote. The discussion of tests is one of the less satisfactory features of the book.

In summary, this reviewer feels that here is an important review of the field of mental deficiency, and a stimulating introduction to ways and means of helping the mentally handicapped to a fuller life. Evidently there is tremendous scope for research in this field, and one hopes that this book will add impetus to the attempts already being made to find solutions to the great variety of social and psychological problems with which mental deficiency confront us.

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Genetic Studies of Genius. V. *The Gifted Group at Mid-Life.* By Lewis M. Terman and Melita H. Oden. Stanford, Calif.: Stanford University Press. 1959. Pp. xiv, 187. $4.50.

Cet ouvrage est une continuation de l'étude fameuse entreprise par Terman et ses collaborateurs, en 1921, sur un groupe de 1,528 enfants

surdoués alors âgés de onze ans en moyenne. Le premier volume, publié en 1925, résumait les caractéristiques mentales et physiques de ce groupe privilégié. Le deuxième de la série, publié en 1926, était une étude collatérale de quelque trois cents de génie reconnus par l'histoire. Deux volumes subséquents, publiés le premier en 1930 et le second en 1947, rapportaient les résultats d'études longitudinales, faites respectivement six ans et dix-huit ans après l'étude originale, sur le même groupe de plus de mille enfants surdoués. Le volume qui fait l'objet de la présente recension décrit ce que sont devenus, trente-cinq ans plus tard, la plupart de ces mêmes sujets. Le fait que plus de 95 pour cent des sujets originaux aient accepté de se prêter à l'expérience indique déjà assez bien avec quel zèle les responsables de ce travail ont su conduire leur investigation et la qualité du rapport qui a pu s'établir entre les enquêteurs et leurs sujets.

Il n'est pas facile de résumer le contenu de cet ouvrage. Les deux premiers chapitres rappellent les techniques employées et les résultats obtenus dans l'étude initiale et dans les deux études subséquentes. Le troisième définit brièvement les limites exactes de l'expérience actuelle et les méthodes qui ont servi à la réaliser. Le rapport porte principalement sur les données recueillies à l'aide de deux questionnaires adressés aux sujets par la poste (le premier en 1950 et le second en 1955). Il fait état également des résultats obtenus par les sujets et leurs conjoints dans une épreuve d'intelligence supérieure (le *Concept Mastery Test*) et par leurs enfants dans le *Stanford-Binet*. Les données recueillies à l'aide d'autres questionnaires (*Supplementary Biographical Data Blank*, *The Happiness of Your Marriage Blank* et *The Rate of Reproduction Blank*) sont aussi utilisées partiellement, mais les auteurs soulignent que l'analyse détaillée de toutes ces données exigera de nouvelles publications. Excepté dans le cas des sujets vivant trop loin de la Californie (environ 18 pour cent), le programme comportait autant que possible des entrevues personnelles avec les sujets eux-mêmes et leurs conjoints. C'est donc à partir de ces techniques diverses qu'ont été explorés successivement la santé physique et mentale des sujets (c. IV), leur niveau mental actuel (c. V), l'état de leur scolarité (c. VI), leur status professionnel (c. VII), leurs intérêts et leurs activités paraprofessionnels (c. VIII), certaines de leurs attitudes sociales et politiques (c. IX) et enfin leur situation maritale et familiale (c. X). Un dernier chapitre résume les précédents et souligne que, dans l'ensemble, la supériorité du groupe se manifeste surtout au plan de la capacité intellectuelle, de la réussite professionnelle et du niveau de scolarité. Au point de vue physique, le groupe reste encore supérieur à la moyenne. Quant à la stabilité émotive et à l'adaptation personnelle, plus difficile à évaluer, les sujets ne semblent pas dépasser tellement la moyenne, même si certains problèmes spécifiques, comme par exemple

l'alcoolisme et l'homosexualité, se rencontrent moins fréquemment chez eux que dans la population générale.

Que faut-il penser d'une telle étude? Il faut bien reconnaître qu'elle représente une somme impressionnante de travail. Le rapport est clairement présenté et copieusement documenté. On ne peut s'empêcher toutefois, à le lire, d'éprouver un certain sentiment de déception ou d'insatisfaction. L'auteur écrit, dans sa préface, que cette recherche apporte des preuves documentées à l'effet que l'enfant surdoué est beaucoup plus apte que l'enfant moyen à devenir un adulte intellectuellement supérieur, bien adapté, et à réussir dans sa profession. A vrai dire, on s'en doutait bien un peu. Si une étude longitudinale de cette nature, mobilisant des ressources financières et un capital humain considérables, ne devaient aboutir qu'à vérifier ce qu'on pouvait prévoir facilement, on peut s'interroger sur la fécondité d'une telle entreprise. Il n'est pas question de minimiser l'importance des études longitudinales. Elles sont presque toujours précieuses et, dans certains cas, rien ne saurait les remplacer. Quand il s'agit, par exemple, d'étudier l'évolution d'un phénomène psychologique, il est pratiquement nécessaire de réexaminer les mêmes sujets à des intervalles successifs. Mais toute étude longitudinale n'est pas nécessairement génétique. Dans le cas actuel, malgré le titre de la série dans laquelle l'ouvrage vient s'insérer, on se trouve en présence d'une étude purement descriptive et factuelle, si largement documentée soit-elle. Pour éclairer le problème posé par l'évolution de l'enfance surdouée, il semble qu'il faudrait autre chose qu'une série d'instantanés, pris à des intervalles relativement considérables et à l'aide d'instruments aussi peu révélateurs que des questionnaires d'information forcément superficiels et toujours sujets à caution. Trop de faits dynamiques risquent ainsi de ne pas apparaître ou de rester sans explication véritable.

En dépit de ces quelques réserves, il reste que cet ouvrage, au niveau auquel il se situe, garde toute sa valeur et constitue un témoignage non équivoque de l'influence que le professeur Terman a exercée et exerce encore sur le développement de la psychologie.

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Figural After-Effects. By PETER MCEWEN. Cambridge: Cambridge University Press [Toronto: Macmillan Company of Canada Limited]. 1958. Pp: 103. $3.85.

SATIATION THEORY IS thought by some to be the most important theory of perception and in terms of the usual criteria employed in evaluating a

theory it comes off very well indeed. Yet it has not been treated in detail in any of the few available books devoted to perception. This monograph provides such a detailed treatment and will prove most valuable to anyone who wants to find in one place a concise, clear statement of the theory and a comprehensive review of the empirical studies which it has stimulated. It will be unfortunate if prospective readers are put off by the title, which gives the impression that the monograph deals only with figural after-effects. A more appropriate title would have mentioned satiation theory specifically since the author's review of perceptual phenomena ranges far wider than figural after-effects and is unified by constant reference to satiation theory.

The contents of the monograph are divided into five chapters, which, for the most part, preserve the historical order of the subject-matter. Chapter 1, "The First Discoveries," briefly describes J. J. Gibson's discovery of figural after-effects and the numerous investigations of this phenomenon that he made. Chapter 2, "The Investigations of Köhler and His Collaborators," contains a brief, clear statement of the essential postulates of satiation theory and reviews a large number of demonstrational experiments on figural after-effects. Reviewed also are studies on the destruction of the Müller-Lyer Illusion and studies employing electrophysiological recordings of brain activity during perception, which have attempted to obtain direct evidence of satiation effects. In Chapter 3, "The Statistical Theory of Figural After-Effects," the theory proposed by Osgood and Heyer as an alternative to satiation theory is described. In this, and to a lesser extent in later chapters, the author presents a nice critical comparison of the experimental predictions which follow from these two theories. Chapter 4, "Other Experimental Studies and Criticisms," reviews a variety of studies on autokinetic movement, reversible figures, visual thresholds, auditory after-effects and other phenomena, as these provide evidence relevant to satiation theory. This chapter, more than any of the others, serves to indicate the potential generality of the theory. Chapter 5, "The Present Status," contains the author's summary of how satiation theory stands in the light of the large body of empirical data reviewed. It comes as no surprise to the reader to find that the theory is not acceptable in its present form and, as all good theories, contains the seeds of its own destruction. The main difficulty is that it is not in accord with known facts of the physiology of the nervous system.

The monograph is clearly organized, concise, and well written. The author is successful in keeping the main theme of his review constantly before the reader, even when examining detailed experimental evidence or elucidating fine theoretical points. To this reviewer then, the monograph represents an excellent piece of scholarly writing, which should be

required reading for anyone actively interested in perception, but not fully familiar with the literature on satiation theory.

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The Undirected Society. By GEOFFREY VICKERS. The University of Toronto Press, 1959. Pp. 162.

Automatic Teaching: The State of the Art. Edited by EUGENE GALANTER. New York: John Wiley & Sons, 1959. Pp. 198.

IS MAN ON THE WAY to a new serfdom? Are machines to be his master? From very different points of view the two books under review have material related to these questions.

The Undirected Society is a record of what Sir Geoffrey Vickers had to say to a Round Table on Man and Industry organized by the School of Social Work of the University of Toronto and extending over the years 1956 to 1958. The central theme of the project was the exploration of the impact of Canada's rapid industrialization on the well-being of the individual. Vickers' essays were intended to provide a conceptual background for the discussions. Neither Sir Geoffrey nor the hundred wise men who met around the round table found any final answers to the questions that started their search. But all were haunted by the same thought—that industrial "progress" was very much a mixed blessing, giving added luxuries but also providing frustrations and de-stabilizing forces which plague mankind.

However, if one does not expect answers, Vickers' essays do provide much material for thought and discussion about the impact of rapid industrialization on Canadian life. The title he gives to the book—*The Undirected Society*—reflects the theme running through the essays—that a society without adequate planning and intelligent control can easily drift into situations that debase human values and frustrate important needs. Man can choose and indeed must choose between economic values such as efficiency and human values such as mental health. This is a stimulating book with a refreshing thread of belief in man's ability to manage his affairs intelligently and with reference to important values.

Automatic Teaching is a collection of papers given at a conference on the Art and Science of the Automatic Teaching of verbal and symbolic skills. As one could expect, such a collection is a hodge-podge of material with very little continuity or organization, about the same as we find in any number of a scientific journal. Twenty contributors express varying degrees of enthusiasm for the use of mechanical teaching devices. There

is much speculation on methods of programming material and some examples of machine programmes for elementary arithmetic, psychology, spelling, and the maintenance of electronic equipment.

On the whole, the group seem to have accepted the basic idea of machine presentation and check-up of verbal material for learning. Most of the discussion revolves around details of methods of presentation and feedback. Kendler raises the central question of transfer which he states has been minimized or ignored by most researchers in the teaching machine area. Pressey raises some major psycho-educational issues and provides a sound basis for the appraisal of the use and value of teaching devices. These two contributions, along with a review of factors in learning efficiency by Gagne and Bolles help to redeem the book from being just a jumble of unimpressive detail.

These two books reflect something of the spirit of our times—man and technology and who is to be the master. We are still searching for the answer in dealing with atomic bombs, teaching machines, and automation. But more and more, people today are turning to human values for guides and away from a materialistic technology that makes process an end in itself.

KARL S. BERNHARDT

University of Toronto

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William Clark Trow, University of Michigan

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480 pages *1959* *$6.00*

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